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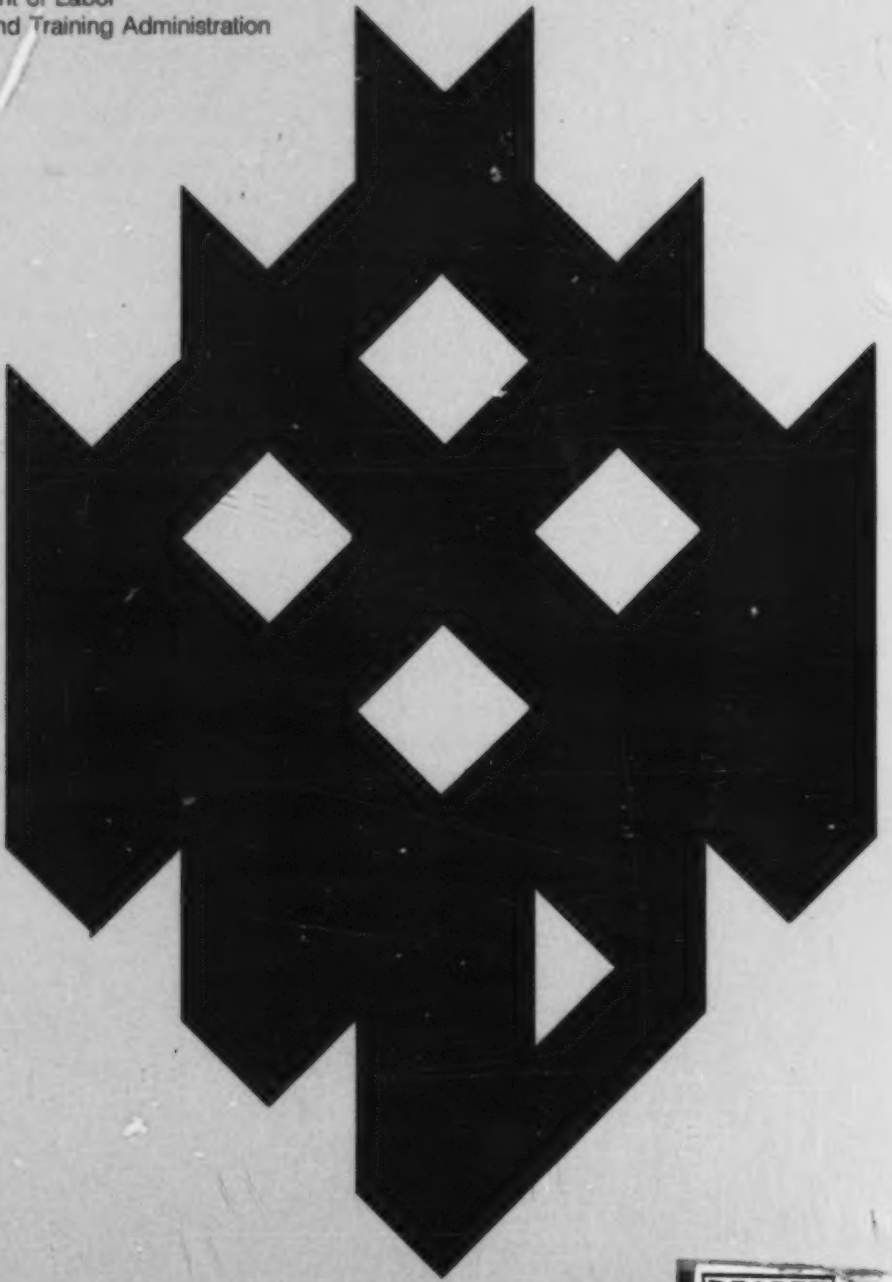
The New Jersey Unemployment Insurance Reemployment Demonstration Project Follow-Up Report

Unemployment Insurance
Occasional Paper 91-1

U.S. Department of Labor
Employment and Training Administration



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U.S. Department of Labor

Employment and Training Administration
Roberts T. Jones
Assistant Secretary of Labor

Unemployment Insurance Service
Mary Ann Wyrsch, Director

1991

This report was prepared for the Unemployment Insurance Service, U.S. Department of Labor and the New Jersey Department of Labor under Contract Number P31948 with Mathematica Policy Research, Inc. The authors of this report are Patricia Anderson, Walter Corson and Paul Decker. Since contractors conducting research and evaluation projects under government sponsorship are encouraged to express their own judgment freely, this report does not necessarily represent the official opinion or policy of the U.S. Department of Labor.

This report is a follow-up to the final report of the New Jersey Unemployment Insurance Reemployment Demonstration Project (UI Occasional Paper 89-3). Original evaluation results were based on impacts measured over a one-to-two year period. The follow-up study extended the evaluation period to nearly four years, thus allowing more definitive measures of demonstration impacts.

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THE NEW JERSEY UNEMPLOYMENT INSURANCE REEMPLOYMENT DEMONSTRATION PROJECT

FOLLOW-UP REPORT

December 1990

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Walter Corson
Project Director

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EXECUTIVE SUMMARY

The purpose of the New Jersey Unemployment Insurance Reemployment Demonstration Project (NJUIRDP) was to examine whether the Unemployment Insurance system could be used to identify displaced workers early in their unemployment spells and to provide them with alternative, early intervention services to accelerate their return to work. Three packages of services, or treatments, were tested in the demonstration: (1) job-search assistance only, (2) job-search assistance combined with training or relocation assistance, and (3) job-search assistance combined with a cash bonus for early reemployment. A key component of the demonstration was that eligible claimants were identified and services were provided through the coordinated efforts of the Unemployment Insurance (UI), Employment Service (ES), and Job Training Partnership Act (JTPA) systems. Another key element was that claimants were required by UI to report for services; failure to report could have led to the denial of benefits.

The demonstration was initiated by the U.S. Department of Labor through a cooperative agreement with the New Jersey Department of Labor. It began operations in July 1986, and, by the end of sample selection in June 1987, 8,675 UI claimants were offered one of the three service packages. Services to eligible claimants were continued into fall 1987 to ensure that all eligibles were able to receive, if desired, the full set of demonstration services. Another 2,385 claimants received existing services in order to provide a control group for comparative purposes for the evaluation. Claimants were assigned randomly to this control group and to the three treatments.

The initial evaluation of the demonstration (Corson et al., 1989) found that each of the treatments reduced UI collections and increased employment and earnings in the year following the UI claim. The initial evaluation also found that the JSA and the reemployment bonus components of the demonstration treatments contributed to the reductions in UI receipt and the increases in earnings, but it did not provide evidence that the training component contributed to these impacts. This result was not surprising, since training was expected only to have longer-run impacts. For this reason, this follow-up study was designed to extend the evaluation to examine UI and earnings impacts over a three- to four-year period beginning with the initial UI claim that led to the selection of claimants in the sample.

The results of the follow-up evaluation indicate that additional reductions in UI receipt occurred in the second year following the UI claim, thereby strengthening the findings of the initial evaluation. No statistically significant long-run effects were found for measures of employment and earnings by treatment group, although some evidence was found to suggest that training may have increased the earnings of trainees. The findings of the follow-up study can be summarized in more detail as follows.

UI RECEIPT AND EMPLOYMENT AND EARNINGS

Each of the treatments reduced the number of weeks of UI collected over the entire follow-up period. These impacts ranged from a reduction of about three-quarters of a week of UI for the JSA-only and JSA plus training and relocation treatments to a reduction of 1.6 weeks of UI for the JSA plus reemployment bonus treatment. This reduction in the number of weeks of UI benefits collected occurred in both the initial benefit year and the following year. The magnitude of the second-year reductions was similar and statistically significant for the JSA-only and JSA plus reemployment bonus treatments. The second-year impacts were not statistically significant for the JSA plus training or relocation treatment. These findings suggest that the JSA component of the treatments led to the longer-term impacts. They also suggest that not only did the JSA component of the treatments lead to more rapid reemployment initially, but it also generated jobs that were more stable than those found by control group members.

·The analysis of employment and earnings suggests that at least one treatment--the JSA plus reemployment bonus treatment--increased earnings initially, but that none of the treatments had longer-run impacts on the probability of working, the amount of earnings, or weeks worked. Employment patterns stabilized by 3 quarters after the initial date of claim, but earnings levels among those who became reemployed remained below base-period levels well after their employment had stabilized. Although reemployed workers were earning more in nominal terms by the 10th quarter than they did before the UI spell, their earnings did not kept pace with inflation, nor with average weekly earnings in manufacturing. These findings suggest that, on average, claimants were unable to obtain reemployment in jobs with the same earnings potential as their pre-UI jobs.

IMPACTS BY TYPE OF CLAIMANT

Participation in training was expected to increase the long-run earnings of trainees, yet comparisons of the earnings impacts of the JSA plus training or relocation treatment with those of the JSA-only treatment suggest that the training component had no additional impact. However, since only a relatively small number of claimants participated in training, the impacts of training would need to be quite large to be detected. Thus, we directly examined the earnings experiences of trainees to determine whether their pattern of earnings suggested that training may have had an impact not detected in the formal analysis. This analysis suggested that both classroom (occupational skills) and on-the-job training did enhance the earnings of trainees.

Because individuals with literacy or language problems were generally not required to participate in demonstration services, we did not expect, and did not find, that the treatments affected their UI receipt or earnings. Nevertheless, the experiences of this group are of interest for future program planners. They suggest that such individuals might benefit from services designed to improve their employment prospects, although they also suggest that this group does not impose a major burden on the UI system.

Our assessment of the impacts of the treatments by subgroups of claimants suggests that the treatments had their greatest impact on individuals who had readily marketable skills and experience. This finding suggests that the treatments should be provided to a wide range of claimants, including those with relatively good reemployment prospects.

BENEFIT-COST ANALYSIS

The results of the benefit-cost analysis suggest that all three treatments offered net benefits to society as a whole and to claimants relative to existing services. The JSA-only treatment and the JSA plus reemployment bonus treatment also led to net gains to the government sector as a whole and to the Labor Department agencies. The JSA plus training or relocation treatment was expensive for the government sector.

These findings suggest that it may be possible to fund the JSA-only and the JSA plus reemployment bonus treatments from the savings in UI benefits and increased UI tax collections. Our estimates indicate that the JSA-only treatment would pay for itself from the perspective of the Labor Department, while the JSA plus reemployment bonus treatment would lead to modest net benefits for the Labor Department. On the other hand, the JSA plus training or relocation treatment could not be funded solely from the savings in UI benefits and increased UI tax collections. It would require either that funding for other programs be reduced or that taxes be increased, since it appears to create net costs to the government as a whole.

I. INTRODUCTION

The purpose of the New Jersey Unemployment Insurance Reemployment Demonstration Project (NJUIRDP) was to examine whether the Unemployment Insurance system could be used to identify displaced workers early in their unemployment spells and to provide them with alternative, early intervention services to accelerate their return to work. Three packages of services, or treatments, were tested in the demonstration: (1) job-search assistance only, (2) job-search assistance combined with training or relocation assistance, and (3) job-search assistance combined with a cash bonus for early reemployment. A key component of the demonstration was that eligible claimants were identified and services were provided through the coordinated efforts of the Unemployment Insurance (UI), Employment Service (ES), and Job Training Partnership Act (JTPA) systems. Another key element was that claimants were required by UI to report for services; failure to report could have led to the denial of benefits.

The demonstration was initiated by the U.S. Department of Labor through a cooperative agreement with the New Jersey Department of Labor. It began operations in July 1986, and, by the end of sample selection in June 1987, 8,675 UI claimants were offered one of the three service packages. Services to eligible claimants were continued into fall 1987 to ensure that all eligibles were able to receive, if desired, the full set of demonstration services. Another 2,385 claimants, who received existing services, were selected to provide a control group for comparative purposes for the evaluation. All claimants were assigned randomly to the three treatments and to the control group.

The initial evaluation of the demonstration (Corson et al., 1989) found that each of the treatments reduced UI collections and increased employment and earnings in the year following the UI claim. The evaluation also found that all three treatments offered net benefits to society over this period when compared with existing services. The JSA-only and JSA plus reemployment bonus treatments also led to net gains to the government.

The initial evaluation did not, however, find evidence that the training component of the second treatment contributed to increased earnings. This finding was not surprising since training was only expected to have longer run impacts. For this reason this follow-up study extends the evaluation of UI and earnings impacts over a longer period than was considered in the initial evaluation. More specifically, the follow-up study examines the impacts over an approximately three to four-year period beginning with the initial UI claim that led to individuals' selection in the sample. The results of the follow-up evaluation indicate that two of the demonstration treatments--JSA-only and JSA plus reemployment bonus--led to statistically significant reductions in UI collections during the second year following the initial claim. These additional reductions in UI payments led to more favorable benefit-cost comparisons than were found in the initial evaluation. No statistically significant long-run effects were found for measures of employment and earnings by treatment group, although some evidence was found to suggest that training may have increased the earnings of trainees.

The remainder of this chapter provides a brief summary of the NJUIRDP design, a summary of the findings of the initial evaluation, and a discussion of the purpose and design of the follow-up study. A final section provides an outline for the remainder of the report.

A. SUMMARY OF THE NJUIRDP DESIGN

The NJUIRDP addressed three objectives: (1) to examine the extent to which UI claimants who might benefit from the provision of employment services could be identified early in their unemployment spells; (2) to assess the policies and adjustment strategies that were effective at helping such workers become reemployed; and (3) to examine how such a UI reemployment program should be implemented. To achieve these objectives, the design called for identifying demonstration-eligible individuals in the week following their first UI payment, and assigning eligible individuals randomly to three treatment groups that were offered alternative packages of reemployment services, and to a control group that received existing services. The demonstration was implemented in 10 sites, corresponding to state UI offices. The sites were chosen randomly, with the probability of their selection proportional to the size of the UI population in each office.

1. Definition of Eligibility

The purpose of the demonstration was to provide reemployment services to experienced workers who, having become unemployed through no fault of their own, were likely to face prolonged spells of unemployment. Their job-finding difficulties might be due to the unavailability of jobs, a mismatch between their skills and job requirements, or their lack of job-finding skills. However, because previous research efforts had failed to establish good predictors of prolonged unemployment spells, complex eligibility requirements could not be used to channel demonstration services. Faced with this situation, the demonstration plan incorporated a small number of sample screens to identify experienced workers who were likely to be displaced permanently from their jobs.

The following eligibility screens were chosen for the demonstration:

1. First Payment. The demonstration excluded claimants who did not receive a first UI payment. To promote early intervention, the demonstration also excluded claimants who did not receive a first payment within five weeks after the initial claim. Individuals who were working and, consequently, who received a partial first payment were also excluded, since their job attachment meant that they had not necessarily been displaced. Finally, claims of a "special" nature (for example, unemployment compensation for ex-service members, unemployment compensation for federal civilian employees, interstate claims, combined wage claims, etc.) were also excluded.

2. Age. An age screen was applied to eliminate the broad category of young workers who have traditionally shown limited attachment to the labor market and whose employment problems may be quite different from older, experienced workers. This screen was set so that workers younger than 25 years of age were excluded from the demonstration.

3. Tenure. It was decided that demonstration-eligible claimants should have exhibited a substantial attachment to a job (or at least to have worked) so that the loss of a job was associated with one or more of the reemployment difficulties described earlier.

This decision was implemented by requiring each claimant to have worked for his or her last employer for three years prior to applying for UI benefits and not to have worked full-time for any other employer during the three-year period. The three-year requirement is used by USDOL's Bureau of Labor Statistics to define displaced workers (Flaim and Sehgal, 1985).

4. Temporary Layoffs. The demonstration treatments were not intended for workers who were facing only temporary layoffs. Thus, it was desirable that claimants on temporary layoff be excluded. However, previous research and experience show that some individuals report that they expect to be recalled even when their chances of actual recall are slim. In order not to exclude such individuals from demonstration services, only individuals who both expected to be recalled and had a specific recall date were excluded.

5. Union Hiring-Hall Arrangement. Individuals who are typically hired through union hiring halls exhibit a unique attachment to a specific labor market and were thus excluded from the demonstration.

2. The Treatments

As stated earlier, the demonstration tested three treatment packages for enhancing reemployment. Eligible claimants were assigned randomly to the three treatment groups (job-search assistance (JSA) only, JSA plus training or relocation, and JSA plus a reemployment bonus) and to a control group which received services that were then currently available.

All three treatments began with a common set of initial components (notification, orientation, testing, a job-search workshop, and an assessment/counseling interview), which were delivered sequentially early in the claimants' unemployment spells. First, a notification letter was sent to claimants after they received their first payment, which occurred about the third week after they filed their claims; thus, claimants usually began to receive services during their fifth week of unemployment. These services began when they reported to a demonstration office (usually an ES office) and received orientation and testing during the same week. In the following week, they attended a job-search workshop consisting of five half-day sessions, and a follow-up, one-on-one counseling/assessment session scheduled for the subsequent week. These initial treatment components were mandatory; failure to report could have led to the denial of UI benefits.

Beginning with the assessment/counseling interview, the nature of the three treatments differed. In the first treatment group--job-search assistance (JSA) only--claimants were told that as long as they continued to collect UI they were expected to maintain periodic contact with the demonstration office, either directly with staff to discuss their job-search activities or by engaging in search-related activities at a resource center situated in the office. The resource center contained job-search materials and equipment, such as job listings, telephones, and occupational and training literature. Claimants were encouraged to use the resource center actively, and were told that if they did not come to the office periodically they would be contacted by ES staff and asked to do so. These periodic follow-up contacts were to occur at 2, 4, 8, 12, and 16 weeks following the assessment interview. ES staff were expected to notify UI when a claimant did not report for services.

Claimants in the second treatment group--JSA plus training or relocation--were also informed about the resource center and of their obligation to maintain contact during their job-search period. In addition, they were told about the availability of classroom and on-the-job training, and they were encouraged to pursue training if interested. Staff from the local JTPA Service Delivery Area (SDA) program operator worked directly with these claimants to pursue the training options. These claimants were also told about the availability of relocation assistance, which, if they elected not to pursue training, they could use to pay for out-of-area job search and for moving expenses.

Claimants in the third treatment group--JSA plus a reemployment bonus--were offered the same set of JSA services as was the first treatment group, but also a bonus for rapid reemployment. The maximum bonus equalled one-half of the claimant's remaining UI entitlement at the time of the assessment interview. This amount was available to the claimant if he or she started working either during the assessment week or in the next two weeks. Thereafter, the potential bonus declined at a rate of 10 percent of the original amount per week until it was no longer available. Claimants could not receive a bonus if they were recalled by their former employer, if the job was with a relative, or if the job was temporary, seasonal, or part-time. They received 60 percent of the bonus if they were employed for 4 weeks, and the remainder if they were employed for 12 weeks.

Each of these treatments tested a different view of the employment problems faced by displaced workers. The JSA-only treatment was based on the assumption that many displaced workers have marketable skills but do not have sufficient job-search experience to identify these skills and sell them in the job market. In contrast, the training treatment was based on the assumption that the skills of some workers are outmoded and must be upgraded. Finally, the reemployment bonus treatment was based on the assumption that JSA alone is an insufficient incentive for claimants to obtain employment rapidly, and that an additional incentive will help them recognize the realities of the job market and accept a suitable job more rapidly than would otherwise be the case.

With the exception of the reemployment bonus and relocation assistance, the services that were offered in the demonstration were similar to those that were available under the existing ES and JTPA systems in New Jersey. However, the likelihood that a claimant was offered and received these services in the demonstration was considerably greater than under the existing system. Moreover, the timing of service receipt also differed: demonstration services were generally provided earlier in the unemployment spell than were existing services.[1]

3. The Provision of Demonstration Services

An important objective of the demonstration was to examine how a reemployment program targeted toward UI claimants should be implemented. Two aspects of that objective were emphasized heavily in the demonstration design phase: (1) using existing agencies and vendors to provide the services, and (2) using a computer-based participant tracking system to facilitate the delivery of services.

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[1] See Corson et al. (1989) for a further discussion of these issues.

In the NJUIRDP, the first aspect meant that the UI agency, the ES, and JTPA's local program operators were all involved in delivering services, and that strengthening linkages among these agencies was an important component of the demonstration. UI staff were responsible for collecting the data that were used to select eligible claimants, and for monitoring compliance by claimants with the demonstration's reporting requirements. A determination of UI eligibility was to be performed when claimants did not report for the initial mandatory services, and, if appropriate, benefits were to be denied.

The initial reemployment services, together with the additional services offered at the assessment/counseling interview, were provided in each demonstration office by a four-person team. This team consisted of three ES staff members and a JTPA staff member from the local SDA program operator. An ES counselor was the team leader and had overall responsibility for ensuring that services were provided. ES staff provided all of the services for the JSA-only and JSA plus reemployment bonus treatment group members. JTPA staff members were involved only with the JSA plus training/relocation treatment group members. They were expected to become involved with the claimants during the assessment/counseling interview and to work with individuals who were interested in classroom or on-the-job training to identify appropriate opportunities and to place the claimants in them. The goal was to use the training opportunities available in each local JTPA SDA. Thus, this component of the demonstration strengthened the linkages between the ES and the local JTPA program operators in the ten demonstration sites.

The other important aspect of the implementation of the demonstration was the extensive use of a computer-based tracking system to operate the program. Data on service delivery were entered into the system, and local office staff were provided with weekly lists of claimants who were expected to receive services. A list of claimants who did not report for services was also generated for use by UI, and monitoring reports were provided to central office staff. The system helped ensure that services were delivered as specified, and that claimants were not "lost" from the program.

4. The Economic Environment

During the demonstration period, the New Jersey economy was experiencing the displacement of workers generated by a long-term secular decline in manufacturing, while substantial growth was occurring in other sectors. Overall, the state economy was quite strong, and the unemployment rate during the demonstration period was low (5 percent). The unemployment rate continued to be low (less than 5 percent) during the follow-up period.

B. SUMMARY OF THE INITIAL EVALUATION FINDINGS

The initial evaluation of the demonstration (Corson et al., 1989) determined that the demonstration eligibility screens directed demonstration services to about one-quarter of the UI claimant population. The most important eligibility screen was the tenure requirement, which excluded individuals who had not worked for their pre-UI employer for at least three years. Other important requirements excluded individuals younger than age 25 and individuals with a definite recall date. The net result of applying the eligibility requirements was an eligible population that contained a substantial proportion of individuals whose age, industry of employment, and other characteristics are usually associated with the displaced worker population and with difficulties in becoming reemployed. Moreover, as compared with a sample of individuals who were not eligible for the demonstration, the eligible population experienced, on average, considerably longer periods of UI collection and longer unemployment spells. Thus, the eligibility screens appear to have directed demonstration services toward a population that generally faced reemployment difficulties.

The initial evaluation also found that the demonstration achieved its objectives of providing an increased level of reemployment services to eligible claimants and of providing these services early in the unemployment spells of claimants. Three-quarters of the claimants in the treatment groups attended the initial orientation, and three-quarters of this group continued through the initial set of job-search services to the assessment/counseling interview. The level at which demonstration-eligible claimants received these services was substantially higher than the level at which individuals in the control group received these services from the existing service network.

The evaluation concluded that the demonstration was generally successful at maintaining on-going contact with treatment group members after they received the initial set of services. The rate of training receipt among members of the second treatment group (JSA plus training or relocation) was also higher than the rates observed for comparable groups of claimants whose exposure to training opportunities came through the regular JTPA service environment in New Jersey. However, the rate of training receipt was low in absolute terms, a situation which affects our ability to detect any impacts of training (see further below).[2] About 19 percent of the claimants who were offered the reemployment bonus received it.

In general, the demonstration treatments were expected to hasten reemployment, thereby reducing the amount of UI collected. The potential exception was the JSA plus training or relocation treatment, for which short-run impacts on UI were expected to be lower than for the other treatments because individuals in training would be eligible to continue to collect benefits. Estimates of the impacts of the treatments on UI receipt showed that all three treatments reduced the amount of benefits collected over the initial benefit year, by $87 per claimant for the first treatment, $81 for the second, and $170 for the third. These findings suggested that all of the treatments were successful at reducing the time spent on UI, and that the bonus offer provided an extra incentive to become reemployed. Data on the timing of these impacts indicated that the rate at which individuals exited from the unemployment system increased primarily during the early part of their claim spells--the period in which intensive job-search assistance was provided.

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[2] Few individuals received relocation assistance.

Evidence on the impacts of the treatments on employment and earnings indicates that all three treatments also increased employment and earnings in the year following the initial UI claim. These increases were larger in the first two quarters after the claim filing date than in the following two quarters, and larger for the JSA-only and JSA plus reemployment bonus treatments relative to the JSA plus training treatment. The training offer did not appear to have been a contributing factor to these initial increases in employment and earnings, while the reemployment bonus offer appeared to have had a small effect. Overall, however, these increases appeared to have arisen primarily because the treatments promoted early reemployment through job-search assistance. This early reemployment did not entail any sacrifice in wages. In fact, the treatments appear to have led to modest increases in hourly wage rates on post-UI jobs.

The initial benefit-cost analysis, which was based on one year of information, indicated that all three treatments offered net benefits both to society as a whole and to claimants when compared with existing services. The JSA-only and JSA plus reemployment bonus treatments also led to net gains to the government sector as a whole, although none of the treatments led to net benefits to the Labor Department agencies that actually offered the services. That is, the observed reductions in UI benefits paid to claimants did not by themselves outweigh the net cost of providing additional services. Overall, net benefits were similar for the JSA-only and JSA plus reemployment bonus treatments, while the JSA plus training or relocation treatment was more expensive than the other treatments from all perspectives.

C. THE PURPOSE AND DESIGN OF THE FOLLOW-UP STUDY

The initial evaluation of the NJUIRDP measured the impacts of the treatments over an approximately one-year period. While this follow-up period was expected to be sufficient to detect the impacts of job-search assistance and the reemployment bonus, it was not expected to be sufficient to detect the impacts of training, since they were expected to occur over a longer time period.[3] For this reason, a follow-up study was proposed to extend the evaluation to examine the impacts of the treatments on UI receipt and earnings over three- to four-year period.

To examine the impacts on UI receipt, administrative data were collected on all new, initial claims (benefit years) that were established after the claim that made individuals eligible for the NJUIRDP. New claims with a first payment through May 1990 and all payments made as of mid-June 1990 were included. These data were used to construct variables that describe UI activity by year, with the years defined according to the initial date of the claim that made the individual eligible for the NJUIRDP. For example, for an individual with a claim date in July 1986, the year of the initial claim, or first year, was the period from that July to July 1987, the second year was July 1987 to July 1988, and so on. The variables that describe UI activity that are used in the analysis are whether a claim was established in the year and the benefits and weeks of UI collected on those claims. Thus, the benefits and weeks collected on claims established in, say, the second year could have been received in the subsequent year. Estimates of UI receipt are reported for four years that include the initial claim year, although the measures of the probability of UI receipt and weeks, and dollars collected are truncated for the last year and are thus underestimates.[4]

Data on weeks worked and earnings were obtained from quarterly wage records to examine the impacts on employment and earnings. These records include earnings on all UI-covered jobs in New Jersey, but they exclude any earnings obtained outside the state and any earnings in uncovered employment, such as earnings from self-employment. For this reason, the wage-records earnings are underestimates. The impacts of the treatments are also likely to be underestimated because they were not measured for uncovered or out-of-state earnings.

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[3] Some of the individuals who received training were still in training at the end of the period used in the initial evaluation.

[4] The fourth year data are truncated both because a full year is not available to measure the probability of receipt and because full benefit year data are unavailable for any claims that were established in the fourth year. In addition the third year history is also truncated for individuals who entered the demonstration in May and June of 1987.

For the analysis, quarterly earnings and weeks worked on all jobs were summed by calendar quarters defined relative to the initial date of the claim. In all, data are reported for 10 quarters beginning with the first calendar quarter after the date of the claim.

The follow-up study examines the impacts on UI receipt and earnings for each of the three treatments over the entire follow-up period. It also focuses explicitly on the experience of two groups of claimants--individuals who received training and individuals who were excused from demonstration services due to literacy or language problems. This latter group is of interest since they received few demonstration services.

D. OUTLINE OF THE REPORT

The remainder of this report contains four chapters and an appendix. Chapter II presents our estimates of the impacts of the demonstration on UI receipt by the three treatment groups for four years that include the initial benefit year. It also presents impacts on the earnings of and weeks worked by the three treatment groups over the 10 quarters following the initial date of claim.

Chapter III examines the experience of individuals who received training and the experience of individuals who were excused from demonstration services due to literacy or language problems. It also assesses the impacts by subgroups of claimants.

Chapter IV updates the benefit-cost analysis to consider the impacts over the entire follow-up period. A final chapter discusses the policy implications of the findings. The appendix discusses methodological issues concerning the training impacts reported in Chapter III.

II. IMPACTS ON UI RECEIPT AND EARNINGS

The initial evaluation of the NJUIRDP showed that, as expected, each of the treatments led to a statistically significant reduction in UI receipt during the initial benefit year, as measured by both the amount of benefits and the number of weeks collected. The initial evaluation also found that all three treatments increased employment and earnings in the year following the initial UI claim. These treatment impacts were larger for the JSA-only and JSA plus reemployment bonus treatments relative to the JSA plus training or relocation treatment. The presence of these impacts during the initial benefit year raises the possibility that further impacts might be observed in subsequent years, particularly for the JSA plus training or relocation treatment, which was expected to have longer-run impacts from training after the treatment group members benefitted fully from training.

This chapter investigates the longer-term effects of the treatments based on UI administrative records and quarterly wage records for the treatment and control samples. It concludes that two of the treatments--JSA-only and JSA plus reemployment bonus--had long-term effects on UI collections. Members of these treatment groups had a lower probability of collecting UI, and they collected smaller amounts of UI in the year after the initial claim year than did members of the control group. No long-term effects on employment and earnings were found.

A. IMPACTS ON UI RECEIPT

The demonstration treatments were expected to and did reduce the amount of UI collected in the initial benefit year. However, the expected direction of any longer-term effects of the treatments is uncertain. On the one hand, the initial, or first-year, impacts appeared to have arisen primarily because the treatments promoted early reemployment. For this reason, treatment group members who were subsequently laid-off would have had a higher probability of qualifying for UI and higher potential UI benefits than control group members. On the other hand, if the jobs found by treatment group members were more stable than those found by control group members, the probability of subsequent UI receipt would be lower for treatment group members than for control group members.

To investigate the impacts of the treatments beyond the initial benefit year, we obtained UI administrative records for the treatment and control samples that covered the period from the initial benefit year through June 1990. Since sample members entered the demonstration from July 1986 to June 1987, this time frame provides data for the initial claim year (the first year) and two subsequent years (the second and third years).[1] Limited data are also available for a fourth year. In the discussion that follows, we report the results for all four years, since the application of random assignment procedures in the demonstration means that all treatment and control groups are affected equally by the incomplete fourth-year data. However, because the data for the fourth year are incomplete, our estimates of the mean levels of UI receipt are biased downward for that year.[2]

To estimate the long-term effects of the treatments, we examined UI receipt for the entire follow-up period and by year. The initial benefit year, or first year, was the 364 days beginning with the initial date of the claim, the second year was defined as the next 364 days, and so on.[3] Impacts were estimated with regressions that controlled for the quarter of enrollment in the demonstration; sex, race, and age; base period earnings; industry; use of a union hiring hall; expectation of recall; potential UI duration; weekly benefit amount; and local office.[4] The tables that report the results show the impacts on treatment group members and the control group means. Treatment group means may be estimated by adding the impact to the control mean.

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[1] The third year data are truncated for individuals who entered the demonstration in May or June 1987.

[2] In addition, we have no information on any UI received by sample members from states other than New Jersey. As with the fourth-year data, our estimates of UI receipt are probably biased downward. However, it is unlikely that treatment-control differences are affected to any great degree, since we have no reason to believe that there are treatment-control differences in state residence.

[3] The New Jersey UI benefit year is 364 days.

[4] These variables were defined as of the date of enrollment in the demonstration.

Tables II.1 and II.2 report the estimated impacts of the treatments on the amount of benefits received and the number of weeks of payments. As can be seen for the entire follow-up period, the JSA-only and JSA plus reemployment bonus treatments had statistically significant impacts on benefits, and all three treatments had statistically significant impacts on weeks of benefits collected. In dollar terms, these overall impacts ranged from a reduction of $107 for the JSA plus training or relocation treatment to a reduction of $293 for the JSA plus reemployment bonus treatment. The weeks collected impacts ranged from a reduction of about three-quarters of a week for the JSA-only and JSA plus training or relocation treatments to a reduction of 1.6 weeks for the JSA plus reemployment bonus treatment.

These impacts for the entire follow-up period were larger than the impacts that were found for the year of the initial claim. The impact estimates by year indicate that the overall results were larger because the amount of UI received declined not only in the year of the initial claim but also in the year following the initial claim. These second-year impacts were statistically significant for the JSA-only and JSA plus reemployment bonus treatments. Interestingly, they were roughly the same size as the impact observed in the first year for the JSA-only treatment. There were no statistically significant impacts in later years, indicating that the long-term effects of the treatments faded after the second year.

Table II.3 presents further information on the nature of the UI impacts. It provides estimates of the impacts on the yearly probability of collecting UI and on the number of benefit years established after the demonstration. These estimates show that the reduction in UI benefits was achieved in part through a reduction in the probability of receiving UI.[5] More specifically, the JSA-only and JSA plus reemployment bonus treatments led to a statistically significant reduction

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[5] The reduction in the probability of UI receipt appears to explain about two-thirds of the reduction in UI receipt in the second year.

TABLE II.1

IMPACTS OF THE TREATMENTS ON UI DOLLARS RECEIVED
(Standard Error in Parentheses)

	JSA-Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group Mean
Year of Initial Claim (First Year)	-87 * (46)	-81 ** (41)	-170 *** (45)	3,228
Second Year	-93 *** (36)	-39 (33)	-78 ** (36)	600
Third Year	0 (38)	-5 (34)	-61 (38)	533
Fourth Year	33 (23)	20 (21)	18 (23)	195
Total	-150 * (83)	-107 (75)	-293 *** (83)	4,559
Total After Initial Claim Year	-63 (69)	-26 (63)	-123 * (69)	1,331

NOTE: The sample used for this analysis includes 2,416 JSA-only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members. The estimates are regression-adjusted treatment-control differences.

* Statistically significant at the 90 percent confidence level for a two-tailed test.
** Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE II.2

IMPACTS OF THE TREATMENTS ON UI WEEKS PAID
(Standard Error in Parentheses)

	JSA-Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group Mean
Year of Initial Claim (First Year)	-0.47 ** (0.24)	-0.48 ** (0.22)	-0.97 *** (0.24)	17.9
Second Year	-0.53 *** (0.19)	-0.22 (0.17)	-0.44 ** (0.19)	3.3
Third Year	0.06 (0.19)	-0.06 (0.17)	-0.30 (0.19)	2.8
Fourth Year	0.19 (0.12)	0.05 (0.10)	0.12 (0.12)	1.0
Total	-0.76 * (0.44)	-0.72 * (0.39)	-1.60 *** (0.43)	25.1
Total After Initial Claim Year	-0.29 (0.36)	-0.24 (0.32)	-0.63 * (0.35)	7.2

NOTE: The sample used for this analysis includes 2,416 JSA-only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members. The estimates are regression-adjusted treatment-control differences.

* Statistically significant at the 90 percent confidence level for a two-tailed test.
** Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE II.3

IMPACTS OF THE TREATMENTS ON THE PROBABILITY OF UI RECEIPT
(Standard Error in Parentheses)

Probability of Receipt	JSA-Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group Mean
Second Year	-0.021 * (0.011)	-0.011 (0.010)	-0.019 * (0.011)	0.22
Third Year	-0.005 (0.010)	-0.006 (0.009)	-0.017 * (0.010)	0.18
Fourth Year	0.005 (0.008)	-0.003 (0.007)	0.000 (0.008)	0.09
Total Number of Claims After Initial Claim	-0.022 (0.021)	-0.020 (0.019)	-0.037 * (0.021)	0.49

NOTE: The sample used for this analysis includes 2,416 JSA-only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members. The estimates are regression-adjusted treatment-control differences.

* Statistically significant at the 90 percent confidence level for a two-tailed test.
** Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

in the probability of receiving UI in the second year. A statistically significant reduction in the probability of receiving UI was also found for the third year for the JSA plus reemployment bonus group. The number of claims over the entire follow-up period also declined for each treatment, but the impact was statistically significant only for the JSA plus reemployment bonus.

Overall, the NJUIRDP treatments appear to have reduced the amount of UI benefits received both in the initial benefit year and in the following year. The magnitude of the second-year reductions was similar and statistically significant for the JSA-only and JSA plus reemployment bonus treatments. The second-year impacts were not statistically significant for the JSA plus training or relocation treatment. These findings suggest that the JSA component of the treatments led to the longer-term impacts. They also suggest that the JSA component of the treatments not only led to more rapid reemployment initially, but also generated jobs that were more stable than those found by control group members.

B. IMPACTS ON EARNINGS

The initial evaluation of the demonstration showed that each of the treatments increased earnings in the year following the initial UI claim, and that these earnings increases were concentrated in the first two quarters following the initial claim. The earnings impacts were also found to be lowest for the JSA plus training or relocation treatment during this time period. This result was expected, since participation in training necessarily reduced the time available for employment. Any impacts of training on earnings were expected to occur over a longer period of time, after training was completed.

These results were obtained on the basis of employment and earnings data that were collected from a survey of sample members and which permitted a detailed investigation of the timing of the impacts. In particular, this analysis focused on quarterly earnings, with the quarters defined relative to the initial UI date of claim. An additional analysis was also performed on the basis of quarterly wage records. Since these data are collected on a calendar basis, this analysis could focus only on calendar quarters that began after the date of claim. The analysis found that the JSA plus reemployment bonus treatment had a statistically significant impact on earnings in the first quarter following the claim, but that the impacts of the other treatments, while positive, were not statistically significant.[6]

In order to investigate whether the JSA plus training or relocation treatment or the other treatments led to longer-run impacts, we collected quarterly wage records through the first quarter of 1990. These records provided 10 calendar quarters of earnings and weeks worked data for all members of the sample beginning with the first calendar quarter following the date of claim. The impacts of the treatments were estimated for these 10 quarters in the same manner as the UI impacts were estimated--with a regression that controlled for the quarter of enrollment in the demonstration; sex, race, and age; base period earnings; industry; use of a union hiring hall; expectation of recall; potential duration; weekly benefit amount; and local office.

Tables II.4 to II.6 report the results of this analysis for the probability of working, earnings, and weeks worked. As shown in the tables, the only impact that is statistically significant for all the measures of employment and earnings is the one reported above--that is, the JSA plus reemployment bonus treatment increased the probability of working (by .04), earnings (by $176), and weeks worked (by .37) in the first quarter following the date of claim. The JSA plus reemployment bonus treatment also led to a statistically significant increase in weeks worked in the second quarter following the date of claim.[7]

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[6] The difference in the findings from the two data sources could arise for a number of reasons, including differences in how the quarters were defined, misreporting on the survey, or the fact that wage records are available for UI-covered employment only in New Jersey. While we have no reason to suspect that the treatments had an impact on the probability of working in covered employment in New Jersey, the unavailability of wage-records data on uncovered jobs and on jobs outside of New Jersey itself is likely to bias the impact estimates downward, since using wage-records data involves the implicit assumption that the treatment-control difference in uncovered employment is zero.

[7] The JSA-only treatment had a statistically significant impact on earnings in quarter 6, but this impact may be due more to a temporary dip in the control group's earnings than to any real treatment impact.

TABLE II.4

IMPACTS OF THE TREATMENTS ON THE PROBABILITY OF WORKING BY QUARTER[a]

(Standard Error in Parentheses)

Quarter of Claim	JSA-Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group Mean
1	0.018 (0.014)	0.015 (0.013)	0.040 *** (0.014)	0.49
2	0.021 (0.014)	0.002 (0.012)	0.022 (0.014)	0.57
3	0.012 (0.014)	0.005 (0.012)	0.003 (0.013)	0.63
4	0.005 (0.014)	-0.004 (0.012)	-0.006 (0.014)	0.63
5	0.003 (0.014)	-0.007 (0.012)	-0.015 (0.014)	0.63
6	0.008 (0.014)	0.004 (0.012)	-0.003 (0.014)	0.60
7	-0.001 (0.014)	-0.010 (0.012)	-0.009 (0.014)	0.64
8	0.004 (0.014)	-0.016 (0.012)	-0.010 (0.014)	0.62
9	0.015 (0.014)	0.007 (0.013)	0.001 (0.014)	0.60
10	0.005 (0.014)	-0.005 (0.012)	-0.010 (0.014)	0.61

NOTE: The sample used for this analysis includes 2,416 JSA-only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members. The estimates are regression-adjusted treatment-control differences.

[a] The probability of working is defined as the probability of having earnings reported in a quarter.

* Statistically significant at the 90 percent confidence level for a two-tailed test.
** Statistically significant at the 95 percent confidence level for a two-tailed test.
*** Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE II.5

IMPACTS OF THE TREATMENTS ON QUARTERLY EARNINGS
(Dollars)
(Standard Error in Parentheses)

Quarter After Claim	JSA-Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group Mean
1	28 (83)	58 (75)	176 ** (88)	1,638
2	75 (90)	-23 (81)	79 (89)	2,174
3	101 (82)	47 (75)	46 (82)	2,507
4	31 (86)	28 (77)	79 (85)	2,517
5	41 (87)	-37 (79)	-83 (87)	2,701
6	223 ** (96)	66 (87)	83 (96)	2,657
7	69 (101)	-45 (91)	-0 (100)	2,970
8	-54 (104)	-133 (94)	-21 (103)	2,924
9	-28 (98)	-33 (89)	-47 (98)	2,911
10	68 (104)	94 (94)	49 (104)	2,951

NOTE: The sample used for this analysis includes 2,416 JSA-only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members. The estimates are regression-adjusted treatment-control differences.

* Statistically significant at the 90 percent confidence level for a two-tailed test.
** Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE II.6

IMPACTS OF THE TREATMENTS ON QUARTERLY WEEKS WORKED
(Standard Error in Parentheses)

Quarter After Claim	JSA-Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group Mean
1	0.18 (0.14)	0.12 (0.13)	0.37 *** (0.14)	3.9
2	0.21 (0.16)	-0.01 (0.14)	0.27 * (0.16)	5.6
3	0.21 (0.16)	0.08 (0.15)	0.08 (0.16)	6.7
4	0.09 (0.17)	-0.02 (0.15)	0.05 (0.17)	6.6
5	-0.03 (0.17)	-0.15 (0.15)	-0.21 (0.17)	6.9
6	-0.24 * (0.17)	0.03 (0.15)	0.02 (0.17)	6.7
7	0.19 (0.17)	-0.09 (0.15)	-0.04 (0.17)	7.2
8	0.10 (0.17)	-0.09 (0.15)	0.06 (0.17)	6.8
9	0.14 (0.17)	0.09 (0.15)	0.12 (0.17)	6.6
10	0.13 (0.17)	0.04 (0.15)	0.02 (0.17)	6.9

NOTE: The sample used for this analysis includes 2,416 JSA-only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members. The estimates are regression-adjusted treatment-control differences.

* Statistically significant at the 90 percent confidence level for a two-tailed test.
** Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

Although there are few impacts to report, it is useful to investigate the pattern of employment and earnings over time. This investigation (using the control group means) indicates that less than 50 percent of control group sample members were employed in covered employment in New Jersey in the first quarter after the claim.[8] This percentage rose substantially in the second quarter to 57 percent, and it stabilized at just over 60 percent by the third quarter. The earnings and weeks worked data show a similar pattern of growth over the first two quarters, followed by relative stability.

Further insights into the employment experiences of claimants can be obtained by examining the pattern of post-UI quarterly earnings relative to quarterly base-period earnings for individuals who were employed in covered employment.[9] This analysis (see Table II.7) shows that the pattern of earnings recovery was similar across treatment groups. In the first quarter after the initial claim, quarterly earnings were well below those for the base period, since many claimants ended their UI spell within the quarter and thus did not work the entire quarter. In the second quarter, more individuals were working the full quarter, and the difference declined. By the third quarter, the average difference was -\$446. As reported earlier, employment probabilities and weeks worked began to stabilize in the third quarter, and, for this reason, there was little change in the fourth quarter, with an average difference of -\$425. By this point, most individuals were working the full quarter, and the negative difference indicates that, on average, claimants were in lower-paying jobs than they were prior to the initial UI claim.

Data for the remaining quarters show that average earnings for employed individuals did not reach pre-UI levels until roughly the 6th quarter after the initial claim. By the 10th quarter, the claimants' earnings had surpassed the base-period level by an average of \$328, an amount which

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[8] It is important to remember that, due to missing wage records for those in uncovered jobs or in covered jobs outside of New Jersey, the proportion employed and the earnings estimates are biased downward.

[9] The variable used for this analysis is defined as quarterly earnings minus average quarterly earnings during the base period, conditional on the presence of earnings in the quarter.

TABLE II.7

QUARTERLY EARNINGS MINUS AVERAGE BASE-PERIOD QUARTERLY EARNINGS FOR EMPLOYED INDIVIDUALS

(Dollars)
(Standard Deviation in Parentheses)

Quarter After Claim	JSA-Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group
1	-1,317 (3,055)	-1,371 (3,086)	-1,406 (3,029)	-1,291 (2,801)
2	-783 (2,941)	-768 (2,792)	-700 (2,779)	-608 (2,754)
3	-481 (2,765)	-408 (2,707)	-475 (2,625)	-419 (2,619)
4	-510 (2,762)	-402 (2,752)	-375 (2,538)	-413 (2,347)
5	-227 (2,642)	-236 (2,656)	-257 (2,605)	-151 (2,596)
6	75 (2,840)	-24 (2,782)	-20 (2,702)	5 (2,514)
7	180 (2,576)	127 (2,706)	114 (2,777)	119 (2,654)
8	-0 (2,874)	69 (2,716)	70 (2,614)	17 (2,670)
9	119 (2,801)	235 (2,869)	162 (2,729)	84 (2,719)
10	331 (2,828)	438 (2,891)	338 (2,783)	205 (2,802)
Base Period Average Quarterly Earnings	4,485 (3,042)	4,525 (2,975)	4,551 (2,866)	4,477 (2,908)

NOTE: The sample used for this analysis includes 2,416 JSA-only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members. The estimates are regression-adjusted treatment-control differences.

* Statistically significant at the 90 percent confidence level for a two-tailed test.
** Statistically significant at the 95 percent confidence level for a two-tailed test.
*** Statistically significant at the 99 percent confidence level for a two-tailed test.

was roughly 7 percent higher than the average base-period earnings of $4,509 per quarter. However, this 7 percent increase in nominal earnings did not keep pace with inflation (the CPI for the Northeast rose approximately 16 percent), nor with the average weekly earnings of manufacturing workers in New Jersey (average weekly earnings rose by approximately 11 percent).[10]

In summary, our wage-records-based analysis of employment and earnings following the initial UI claim suggests that at least one treatment, the JSA plus reemployment bonus treatment, increased earnings initially, but that none of the treatments had longer-run impacts on the probability of working, the amount of earnings, or weeks worked. Our examination of the pattern of employment suggests that employment patterns stabilized by 3 quarters after the initial date of claim. This investigation also shows that earnings levels among those reemployed remained below base-period levels well after employment had stabilized. Although reemployed workers were earning more in nominal terms by the 10th quarter than they did before the UI spell, their earnings did not kept pace with inflation, nor with average weekly earnings in manufacturing. These findings suggest that, on average, claimants were unable to obtain reemployment in jobs with the same earnings potential as their pre-UI jobs.

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[10] These comparisons were made from April 1986, the mid-point of the average individual's base period, to May 1989, the mid-point of the 10th quarter after the initial claim for the average sample member.

III. UI RECEIPT AND EARNINGS BY TYPE OF CLAIMANT

This chapter extends the analysis in the previous chapter by examining the UI receipt and employment and earnings experience of several groups of claimants. The first two sections examine the experience of two groups who are of special interest--individuals who received training, and individuals with literacy or language problems. Individuals who received training are of interest because the training they received was expected to enable them to increase their earnings after they completed the training, yet the analysis of the differences among treatment groups in the previous chapter did not find such impacts. However, the analysis presented here suggests that training enhanced the economic position of claimants who participated in classroom or on-the-job training.

Because individuals with literacy or language problems were generally not required to participate in demonstration services, we did not expect that the treatments would affect their UI receipt or earnings. Indeed, we found that the treatments did not. Nevertheless, the experience of this group is of interest for future program planners. It suggests that such individuals might benefit from services designed to improve their employment prospects, although it also indicates that these individuals do not impose a major burden on the UI system.

The final section of the chapter examines the impacts of the treatments more broadly by type of claimant to assess the degree to which various subgroups contributed to the overall impacts reported in the previous chapter. It concludes that, as we found in the initial evaluation, the treatments appear to have had their greatest impact on individuals who had readily marketable skills and experience.

A. INDIVIDUALS WHO RECEIVED TRAINING

In Chapter II, we found that the impacts of the JSA plus training or relocation treatment on UI and earnings were not significantly greater than the impacts of the JSA-only treatment.

Additional comparisons between individuals who were assessed and were offered training (in the JSA plus training or relocation treatment) and individuals who were assessed and not offered training (in the JSA-only treatment) also do not reveal statistically significant differences between the two treatments. These findings suggest that, on average, the training component of the JSA plus training or relocation treatment had no additional impact on UI receipt or earnings beyond the impact of the JSA services. However, this conclusion may be misleading. Because only a small percentage (15 percent) of individuals who were offered training actually received training, the impacts of training would need to be quite large (on the order of $1,500 a quarter) to be detected.[1] Impacts of this magnitude are much larger than have typically been found in evaluations of training programs (for a summary, see Leigh, 1990). Thus, we examined the UI and earnings experience of trainees directly to determine whether the pattern of UI receipt and earnings suggests that the training may have had an impact that was not detected in the formal analysis of treatment group differences.

As part of our examination of claimants who participated in training, we compared their experience with the experience of the assessed JSA-only claimants to determine how it was affected by training. Individuals who were not assessed could not have received a training offer, regardless of the treatment group to which they were assigned. Hence, by excluding claimants who were not assessed from our analysis, we create a sample of claimants who are more closely comparable to the trainees.

Despite our efforts to create the best possible comparison group, we cannot interpret the benefit, employment, and earnings differences between the trainees and the assessed JSA-only group as estimates of the impact of training, since training participants were not chosen at random. Individuals who participated in training were likely to differ systematically from other individuals in the sample, and we have no equivalent group of nontrainees with which they can be compared as a basis for estimating the impact of training. However, we can compare the experience of trainees with the experience of the assessed JSA-only claimants to provide a general description of the economic status of training recipients in the years following the claim date.

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[1] For example, if we compared the quarterly earnings of assessed claimants in the JSA-only treatment with the quarterly earnings of assessed claimants in the JSA plus training or relocation treatment, the training impacts would need to be at least $1,500 per quarter per trainee to be detected with a 70 percent chance if we used a one-tail test.

We also extend our analysis of employment and earnings outcomes for trainees and nontrainees by using regression methods to control for observed differences between the two groups of claimants, including differences in base period earnings. The regression specification for this analysis includes as the dependent variable the change in earnings between the base period and the post-claim quarter. Hence, we measure the effect of training as the estimated impact on the relative change in earnings between the base period and the post-claim periods.[2]

Following the UI claim date, the labor-market and UI benefit experience of the claimants in the JSA plus training or relocation group who received training appears to have differed significantly from the experience of the claimants who were assigned to the JSA-only group and were assessed. Claimants who received on-the-job training experienced relatively high levels of employment and earnings throughout the period following the initial claim. These claimants also received relatively lower levels of UI benefits than were received by the assessed JSA-only claimants in the four years following the initial claim. On the other hand, claimants who received classroom training (which was occupational training, as opposed to remedial or general education) experienced less employment and lower earnings than the JSA-only claimants in the first three quarters following the claim date. In subsequent quarters, the classroom trainees experienced employment and earnings that were somewhat higher than the employment and earnings of the assessed JSA-only group. The pattern of UI receipt among classroom trainees was consistent with their employment and earnings experience. The classroom trainees received relatively high benefits in the year of their initial claim, but received relatively low benefits in subsequent years. These findings suggest that training participation enhanced the employment and earnings of both on-the-job trainees and classroom trainees in the long-run.

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[2] Ashenfelter (1978) and Card and Sullivan (1988) show that a similar estimator yields a consistent estimate of the impact on earnings if (1) shocks in earnings are uncorrelated with their own lagged values and with the decision to participate in training and (2) the individual-specific component of earnings enters linearly into the earnings equation. The implications of these assumptions are discussed in Appendix A.

1. Earnings and Employment

The 314 claimants in the JSA plus training or relocation group who participated in classroom training received lower earnings on average than the assessed JSA-only group members in the first three quarters following the claim date. Data in Table III.1 show that the earnings of classroom training recipients were significantly lower (at the 99 percent level of confidence) than those of JSA-only claimants in the first three quarters following the claim date. The differences in each of the first three quarters were large--particularly in the second quarter, in which the difference was nearly $700. These differences are not surprising, given the nature of classroom training. Because many individuals did not work while they were attending training, we would expect that many of the training recipients had zero earnings in the early quarters, when they were still participating in the training program or were awaiting job placement after training.

After the third quarter following the claim date, earnings for the classroom trainees were higher than the earnings for the assessed JSA-only group. In general, these differences grew over time, and for the final three quarters of observation the differences were statistically significant at the 90 or 95 percent levels of confidence. These differences suggest that, once training was completed, the earnings levels of classroom trainees were higher than those of claimants who received JSA only and were assessed.

Since the training received during the demonstration varied from very short-term training to longer-term training (6 months or more), we compared the earnings of classroom trainees who participated in training for at least 2 months with the earnings of those who participated for less than 2 months. As one would expect, the earnings of the trainees who received 2 or more months

TABLE III.1

AVERAGE QUARTERLY EARNINGS OF TRAINING RECIPIENTS
(Dollars)
(Standard Deviations in Parentheses)

Quarter	JSA Plus Training or Relocation: Classroom Trainees	JSA Plus Training or Relocation: On-the-Job Trainees	JSA-Only: Assessed Claimants
Base Period (Average per Quarter)	4,662 (2,382)	4,085 * (2,345)	4,735 (2,999)
1	610 *** (1,624)	1,926 ** (2,454)	1,109 (2,805)
2	1,001 *** (2,818)	3,375 *** (2,520)	1,682 (3,314)
3	1,868 ** (2,371)	4,220 *** (3,058)	2,230 (2,858)
4	2,442 (2,616)	4,675 *** (3,640)	2,299 (2,983)
5	2,697 * (2,684)	4,902 *** (3,718)	2,370 (2,813)
6	2,667 (2,691)	4,371 *** (3,630)	2,529 (3,479)
7	3,068 (2,934)	4,827 *** (3,676)	2,802 (4,063)
8	3,170 ** (3,131)	4,794 *** (3,623)	2,720 (3,271)
9	3,083 * (2,987)	4,994 *** (3,600)	2,729 (3,346)
10	3,344 ** (3,705)	5,202 *** (3,545)	2,816 (3,313)
Number of Observations	314	45	1,363

*Significantly different from the JSA-only treatment mean at the 90 percent confidence level in a two-tail test.

**Significantly different from the JSA-only treatment mean at the 95 percent confidence level in a two-tail test.

***Significantly different from the JSA-only treatment mean at the 99 percent confidence level in a two-tail test.

of training were substantially lower in the early quarters than the earnings of the trainees who received less than 2 months of training, as shown in Table III.2. Although this difference became smaller in later quarters, it did not disappear--the longer-duration trainees earned less than the shorter-duration trainees in each quarter. Hence, there is no evidence that the longer-duration trainees received higher earnings than shorter-duration trainees at any point during the period of observation.

Data on the weeks worked by classroom trainees reveal a pattern over time that is similar to the earnings pattern, as shown in Table III.3. In the first two quarters following the claim date, classroom trainees worked significantly fewer weeks per quarter than JSA-only claimants who were assessed. After the first three quarters, classroom trainees tended to work more weeks per quarter on average than did the assessed JSA-only claimants. In quarters 4 through 10, the differences in weeks worked between the two groups were statistically significant at the 95 percent level of confidence.

Our estimates of the impacts of classroom training on the employment and earnings of trainees that control for individual characteristics (Tables III.4 and III.5), also suggest that classroom training reduced employment and earnings in the first two quarters after the initial claim, and then enhanced the employment and earnings of the trainees in later periods. Training program participation reduced both weeks of work and earnings slightly in quarters 1 and 2. Beginning in quarter four, classroom trainees began to work more and to earn more than did the assessed claimants in the JSA-only group. The impact of training was to increase employment by somewhat less than one week per quarter from quarters 4 to 10 and to increase earnings by as much as $500 (quarters 8 and 10). At 8 quarters after the claim date, the impact of training on the earnings of classroom trainees represented nearly 20 percent of the total earnings received by the assessed claimants in the JSA-only group.

TABLE III.2

AVERAGE QUARTERLY EARNINGS OF CLASSROOM TRAINEES, BY DURATION OF TRAINING
(Dollars)
(Standard Deviations in Parentheses)

Quarter	Classroom Trainees with Less Than 2 Months of Training	Classroom Trainees with 2 or More Months of Training	JSA-Only: Assessed Claimants
Base Period (Average per Quarter)	4,960 (2,658)	4,535 (2,249)	4,735 (2,999)
1	722 ** (1,662)	562 *** (1,608)	1,109 (2,805)
2	2,080 (4,602)	540 *** (1,283)	1,682 (3,314)
3	2,407 (2,850)	1,637 *** (2,099)	2,230 (2,858)
4	3,004 ** (3,189)	2,201 (2,296)	2,299 (2,983)
5	3,112 ** (3,287)	2,519 (2,367)	2,370 (2,813)
6	3,245 ** (3,015)	2,420 (2,508)	2,529 (3,479)
7	3,683 ** (3,454)	2,805 (2,647)	2,802 (4,063)
8	3,884 *** (3,856)	2,866 (2,718)	2,720 (3,271)
9	3,579 ** (3,452)	2,870 (2,745)	2,729 (3,346)
10	3,648 ** (3,354)	3,214 (3,846)	2,816 (3,313)
Number of Observations	94	220	1,363

* Significantly different from the JSA-only treatment mean at the 90 percent confidence level in a two-tail test.
** Significantly different from the JSA-only treatment mean at the 95 percent confidence level in a two-tail test.
*** Significantly different from the JSA-only treatment mean at the 99 percent confidence level in a two-tail test.

TABLE III.3

AVERAGE WEEKS WORKED IN QUARTER BY TRAINING RECIPIENTS

(Standard Deviations in Parentheses)

Quarter	JSA Plus Training or Relocation: Classroom Trainees	JSA Plus Training or Relocation: On-the-Job Trainees	JSA-Only: Assessed Claimants
Base Period (Average per Quarter)	11.5 (2.1)	11.2 (2.4)	11.4 (2.2)
1	1.4 *** (3.2)	4.7 *** (4.9)	2.3 (3.9)
2	2.7 *** (4.4)	9.3 *** (4.4)	4.5 (5.4)
3	5.5 (5.6)	10.4 *** (4.8)	6.0 (5.9)
4	7.1 ** (5.8)	10.8 *** (4.3)	6.3 (5.9)
5	7.7 *** (5.9)	10.9 *** (4.5)	6.3 (6.0)
6	7.2 ** (6.1)	10.3 *** (4.6)	6.4 (6.1)
7	7.7 ** (5.9)	10.6 *** (4.5)	6.9 (6.1)
8	7.7 *** (6.0)	10.4 *** (4.4)	6.7 (6.1)
9	7.4 ** (6.0)	10.9 *** (4.5)	6.6 (6.1)
10	7.7 *** (6.1)	11.0 *** (4.4)	6.7 (6.1)
Number of Observations	314	45	1,363

*Significantly different from the JSA-only treatment mean at the 90 percent confidence level in a two-tail test.

**Significantly different from the JSA-only treatment mean at the 95 percent confidence level in a two-tail test.

***Significantly different from the JSA-only treatment mean at the 99 percent confidence level in a two-tail test.

TABLE III.4

ESTIMATED IMPACTS OF TRAINING ON THE AVERAGE QUARTERLY EARNINGS OF TRAINING RECIPIENTS

(Dollars)
(F Statistics in Parentheses)

Quarter	Classroom Training	On-the-Job Training
1	-458 ** (4.96)	1,469 *** (8.67)
2	-635 *** (7.05)	2,347 *** (16.33)
3	-314 (2.20)	2,632 *** (26.21)
4	195 (0.76)	2,995 *** (30.32)
5	384 * (3.15)	3,174 *** (36.63)
6	191 (0.59)	2,480 *** (16.99)
7	323 (1.44)	2,652 *** (16.54)
8	505 ** (4.50)	2,681 *** (21.53)
9	409 * (3.02)	2,932 *** (26.32)
10	582 ** (5.78)	3,005 *** (26.15)

NOTE: The estimated impacts of training are based on regressions that include training indicators and a set of interaction terms, where the interaction terms are equal to the product of the training indicators and a variety of economic and demographic variables. The hypothesis test used to evaluate the statistical significance of the estimated impact is an F test of the linear equation implied by having the training indicators take a value of 1. The test is calculated according to the assumption that the economic and demographic variables are equal to the means for the training groups. The critical values for the F statistic are 2.71, 3.84, and 6.63 for the 90, 95, and 99 percent confidence levels, respectively. Table A.1 in Appendix A contains the full regressions on which these impact estimates are based.

* Significantly different from zero at the 90 percent confidence level in an F test.
** Significantly different from zero at the 95 percent confidence level in an F test.
***Significantly different from zero at the 99 percent confidence level in an F test.

TABLE III.5

ESTIMATED IMPACTS OF TRAINING ON THE AVERAGE WEEKS WORKED PER QUARTER BY TRAINING RECIPIENTS

(F Statistics in Parentheses)

Quarter	Classroom Training	On-the-Job Training
1	-1.00 *** (13.83)	2.57 *** (15.62)
2	-1.90 *** (28.49)	4.98 *** (33.36)
3	-0.60 (2.47)	4.55 *** (23.79)
4	0.74 * (3.64)	4.69 *** (25.07)
5	1.27 *** (10.76)	4.76 *** (25.75)
6	0.67 * (2.83)	4.05 *** (17.80)
7	0.74 * (3.46)	3.88 *** (16.22)
8	0.90 ** (5.36)	3.81 *** (16.14)
9	0.80 ** (4.34)	4.55 *** (23.93)
10	0.85 ** (4.69)	4.38 *** (21.21)

NOTE: The estimated impacts of training are based on regressions that include training indicators and a set of interaction terms, where the interaction terms are equal to the product of the training indicators and a variety of economic and demographic variables. The hypothesis test used to evaluate the statistical significance of the estimated impact is an F test of the linear equation implied by having the training indicators take a value of 1. The test is calculated according to the assumption that the economic and demographic variables are equal to the means for the training groups. The critical values for the F statistic are 2.71, 3.84, and 6.63 for the 90, 95, and 99 percent confidence levels, respectively. Table A.2 in Appendix A contains the full regressions on which these impact estimates are based.

* Significantly different from zero at the 90 percent confidence level in an F test.
** Significantly different from zero at the 95 percent confidence level in an F test.
***Significantly different from zero at the 99 percent confidence level in an F test.

A relatively small number of claimants in the JSA plus training or relocation group (45 individuals) received on-the-job training. Claimants who received on-the-job training had significantly higher earnings than did the assessed JSA-only claimants in all quarters following the first quarter after the claim date, as shown in Table III.1. To some extent, this result is not surprising, because, by definition, on-the-job training recipients should have been employed, at least in the early quarters. However, the higher earnings of on-the-job trainees persisted into the later quarters because the trainees remained employed and their earnings grew over time. By quarter 10 after the claim date, on-the-job trainees were receiving over 25 percent more earnings than they received during the average quarter in the base period. On-the-job trainees replaced their base-period earnings to a much greater extent than did the assessed JSA-only claimants, who received earnings in quarter 10 that were more than one-third lower than their base-period earnings. While this evidence cannot be used to argue that on-the-job training will increase earnings for a randomly chosen group of UI claimants, it does demonstrate that the claimants who received on-the-job training achieved a relatively high level of earnings after the demonstration.

The findings on weeks worked by on-the-job trainees are consistent with the findings on earnings. By the third quarter after the claim date, on-the-job trainees were employed for almost 11 of 13 weeks in that quarter. The trainees maintained just under 11 weeks of work per quarter for the remainder of the observation period. During the same period, the assessed claimants in the JSA-only group were employed for less than 7 weeks per quarter. The difference in weeks of work between the two groups is statistically significant at the 99 percent level of confidence in each of the quarters. These differences in weeks worked were probably responsible for the majority of the earnings differences described above; on-the-job trainees received significantly greater earnings than JSA-only claimants because they worked significantly more weeks.

The regression-based estimated impacts of on-the-job training on employment and earnings are consistent with these differences. Our findings indicate that on-the-job training had a large positive impact on weeks of work and on earnings in each of the postclaim quarters, as shown in Tables III.4 and III.5. The impact on earnings was equal to $2,000 to $3,000 per quarter after quarter 1, and the impact on weeks worked was equal to 4 to 5 weeks per quarter after the first quarter. Both the estimated impacts on earnings and the estimated impacts on weeks worked were statistically significant at the 99 percent level of confidence in each of the postclaim quarters.

2. UI Receipt

The findings on UI receipt by training recipients are generally consistent with the findings on the earnings and employment of this group of claimants. Table III.6 shows that classroom trainees received about $4,500 in benefits in the year of their initial claim, compared with about $3,900 received by assessed JSA-only claimants. Similarly, weeks of UI benefits collected in the year of the initial claim differed between the two groups: classroom trainees received about 24 weeks on average, compared with about 21 weeks on average by the assessed JSA-only claimants. Thus, classroom trainees received greater benefits during the time in which they participated in the training programs. This finding is consistent with the previous finding that the employment and earnings of this group were relatively low during the year following the initial claim.

After the year of the initial claim, classroom trainees received less UI benefits on average than did the assessed JSA-only claimants, in terms of both dollars paid and weeks paid. On average, the overall amount of UI receipt by classroom trainees was about 25 percent lower than the amount received by the assessed JSA-only group over the postdemonstration period. However, the differences in UI receipt by the two groups were statistically significant only in the third year after the initial claim, in which the classroom trainees received slightly more than half of the benefits received by the assessed JSA-only claimants.

The differences in UI benefits receipt by for two groups are consistent with the employment and earnings findings for classroom trainees and the assessed JSA-only claimants. Evidence in Tables III.1 and III.4 suggests that classroom trainees received significantly higher earnings than

TABLE III.6

AVERAGE UI RECEIPT BY TRAINING RECIPIENTS

(Standard Deviations in Parentheses)

	JSA Plus Training or Relocation: Classroom Trainees	JSA Plus Training or Relocation: On-the-Job Trainees	JSA-Only: Assessed Claimants
		UI Benefits	
Total Benefits	5,237 *** (2,209)	3,238 *** (1,946)	4,849 (2,590)
Year of Initial Claim (First Year)	4,512 *** (1,284)	2,587 *** (1,179)	3,896 (1,533)
Second Year	363 (1,179)	229 (789)	370 (1,125)
Third Year	223 *** (885)	353 (1,281)	394 (1,244)
Fourth Year	139 (788)	68 (457)	185 (804)
		Weeks Paid	
Total Weeks	27.6 * (8.7)	18.6 *** (10.3)	26.5 (12.9)
Year of Initial Claim (First Year)	24.1 *** (4.0)	15.5 *** (7.0)	21.3 (6.3)
Second Year	1.7 (5.4)	1.2 (3.9)	2.0 (5.9)
Third Year	1.2 *** (4.7)	1.7 (5.9)	2.2 (6.5)
Fourth Year	0.6 (3.2)	0.2 *** (1.6)	0.9 (4.0)
Number of Observations	314	45	1,363

* Significantly different from the JSA-only treatment mean at the 90 percent confidence level in a two-tail test.

** Significantly different from the JSA-only treatment mean at the 95 percent confidence level in a two-tail test.

*** Significantly different from the JSA-only treatment mean at the 99 percent confidence level in a two-tail test.

did the assessed JSA-only group in quarters 8 to 10, a period which overlaps with the third year as defined in Table III.6. Given the greater employment and earnings of trainees during this period, it is not surprising that they also received significantly less UI benefits.

Claimants in the JSA plus training or relocation group who received on-the-job training received significantly less UI benefits during the year of the initial claim, while the differences in subsequent years were not significant. The difference in weeks of UI paid for the two groups was equal to 6 weeks in the year of the initial claim. Annual differences in subsequent years are all less than one week and are not statistically significant. These findings are relatively consistent with the findings on the employment and earnings of on-the-job trainees. The largest differences in UI receipt occurred in the year of the initial claim, a period in which on-the-job trainees quickly achieved a level of earnings equal to that experienced in the base period. The difference in UI receipt in later years also reflects the greater employment and earnings of the trainees during those years, although the UI differences were not statistically significant.

B. INDIVIDUALS WITH LITERACY OR LANGUAGE PROBLEMS

Individuals with literacy or language problems received few services in the NJUIRDP. For operational reasons, these individuals could not be tested in many instances, and they were frequently exempt from participating in the job-search workshops that were part of each NJUIRDP treatment. In addition, no special services, such as remedial education or English as a Second Language (ESL), were directed explicitly toward these individuals. Although these individuals could have received special services through their contact with the Employment Service or JTPA program operators, available data suggest that few actually did receive remedial education or ESL.

For these reasons, it is unlikely that the NJUIRDP treatments affected the UI or employment outcomes of individuals who had literacy or language problems. Nevertheless, an examination of the UI and employment experiences of these individuals is of interest to determine whether they might benefit from special employment services in future program settings.

Individuals with literacy or language problems cannot explicitly be identified with the data collected for the demonstration. However, because the primary reasons for being excused from testing or the workshops were language or literacy problems, an expectation of recall, and the use of a union hiring hall, we can identify a group which consists largely of those with literacy or language problems by excluding individuals who said that they expected to be recalled or used a union hiring hall at the time they initially applied for UI. In addition, our computation also excluded all claimants from the Jersey City office, since that office adopted an expanded excusal policy.

The procedure yielded a subgroup of 824 claimants, of whom 230 were in the JSA-only treatment group, 377 in the JSA plus training or relocation treatment group, and 217 in the JSA plus reemployment bonus treatment group. Overall, individuals in this group were more likely to be nonwhite (31 percent were Hispanic), older, and employed in durable manufacturing than were individuals in the full demonstration group (Table III.7.)

An examination of data on quarterly earnings, weeks worked, and the probability of working for individuals with literacy or language problems by treatment group shows that employment outcomes did not differ among treatment groups (Tables III 8 and III.9). When compared with our findings in Chapter II, the data also show that the time pattern of employment and earnings for the subgroup with literacy and language problems was fairly similar to the time pattern for the full group of claimants. However, earnings and weeks worked appear to have stabilized at somewhat lower levels than for the full group of claimants, despite the fact that average base-period earnings were similar for the subgroup and the full group.

Data in Table III.9 show that the lower earnings of the subgroup with literacy and language problems were due in part to their lower probability of working relative to other claimants. The probability of working stabilized in the third quarter after the initial claim, just as it did for the full group, but this probability was around 0.53 to 0.56, rather than the 0.60 to 0.63 found earlier.

TABLE III.7

CHARACTERISTICS OF THE LITERACY OR LANGUAGE PROBLEM SUBGROUP COMPARED WITH THE CHARACTERISTICS OF THE REMAINDER OF THE TREATMENT SAMPLE

	Literacy or Language Problem Subgroup	Remainder of the Treatment Sample
Demographic Variables		
Sex (percent)		
Male	57.3	51.6
Female	42.7	48.4
Race (percent)		
White	48.7	62.4
Black	20.0	17.1
Other	31.3	20.5
Age (percent)		
Younger than 35	22.7	30.9
35-55	49.6	47.8
Older than 55	27.7	21.3
Base Period Employment		
Mean Earnings	$18,536	$18,036
Industry (percent)		
Durable manufacturing	25.1	17.6
Nondurable manufacturing	16.6	20.0
Nonmanufacturing	58.3	62.4
UI Entitlement		
Weekly Benefit Amount	$176	$181
Potential Duration (weeks)	25	25
Number of Observations	824	7,851

TABLE III.8

QUARTERLY EARNINGS OF THE LITERACY AND LANGUAGE PROBLEM SUBGROUP
(Standard Deviations in Parentheses)

	JSA-Only		JSA Plus Training or Relocation		JSA Plus Reemployment Bonus	
Quarter of Initial Claim	Earnings (Dollars)	Weeks Worked	Earnings (Dollars)	Weeks Worked	Earnings (Dollars)	Weeks Worked
Base Period Earnings	4,384	11.4	4,726	11.5	4,737	11.4
(Average per Quarter)	(2,802)	(2.6)	(3,439)	(2.9)	(3,384)	(3.0)
1	1,256	3.1	1,162	2.6	1,886	3.6
	(2,907)	(4.9)	(3,294)	(4.4)	(3,311)	(5.2)
2	1,859	5.0	1,722	4.5	2,033	4.8
	(2,958)	(5.9)	(3,382)	(5.7)	(3,265)	(5.8)
3	2,294	6.0	2,025	5.7	2,370	5.8
	(4,238)	(6.7)	(2,925)	(6.0)	(3,328)	(6.0)
4	2,286	6.2	2,222	6.1	2,660	6.5
	(3,030)	(6.6)	(3,060)	(6.3)	(3,503)	(6.4)
5	2,369	6.2	2,465	6.4	2,495	6.3
	(3,035)	(6.6)	(3,953)	(6.4)	(3,120)	(6.2)
6	2,463	6.5	2,350	6.0	2,430	6.1
	(3,118)	(6.6)	(4,036)	(6.1)	(3,109)	(6.1)
7	2,729	7.2	2,513	6.3	2,546	6.5
	(3,249)	(6.4)	(3,517)	(6.2)	(3,101)	(6.4)
8	2,603	6.9	2,359	6.1	2,750	6.5
	(3,226)	(6.6)	(3,182)	(6.0)	(3,448)	(6.3)
9	2,685	6.7	2,442	6.1	2,678	6.3
	(3,367)	(6.6)	(3,284)	(6.1)	(3,632)	(6.6)
10	2,805	6.8	2,330	5.8	2,855	6.3
	(3,417)	(6.6)	(3,289)	(6.1)	(3,738)	(6.3)
Number of Observations	230		377		217	

TABLE III.9

PROBABILITY OF WORKING BY THE LITERACY AND LANGUAGE PROBLEM SUBGROUP

(Standard Deviations in Parentheses)

Quarter After Initial Claim	JSA-Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus
1	0.41 (0.49)	0.38 (0.49)	0.44 (0.50)
2	0.49 (0.50)	0.45 (0.50)	0.49 (0.50)
3	0.51 (0.50)	0.54 (0.50)	0.55 (0.50)
4	0.54 (0.50)	0.55 (0.50)	0.53 (0.50)
5	0.55 (0.50)	0.56 (0.50)	0.56 (0.50)
6	0.54 (0.50)	0.55 (0.50)	0.54 (0.50)
7	0.61 (0.49)	0.55 (0.50)	0.55 (0.50)
8	0.58 (0.50)	0.55 (0.50)	0.53 (0.50)
9	0.57 (0.50)	0.55 (0.50)	0.54 (0.50)
10	0.57 (0.50)	0.52 (0.50)	0.54 (0.50)
Number of Observations	230	377	217

Unfortunately, we cannot determine whether a larger percentage of the subgroup with literacy and language problems than of the full group were unemployed, or whether a larger percentage of the subgroup than the full group did not work in New Jersey covered employment.

Data on the difference between quarterly earnings and average quarterly base-period earnings, conditional on the presence of earnings in a quarter, indicates that members of the subgroup with literacy and language problems who became reemployed also had lower earnings relative to base-period earnings than did their counterparts in the full demonstration group (Table III.10). The nominal quarterly earnings of members of the full group were equal to their base-period earnings by the 6th quarter, while members of the subgroup with literacy and language problems did not reach parity with base-period earnings until later. Data on UI receipt show a similar story (Table III.11). During the year of the initial claim, members of the subgroup with literacy and language problems collected slightly more weeks and dollars of UI benefits than did members of the full group. However, in the following years, their UI receipt was below that of the full group. As shown in the bottom panel of the table, the probability of UI receipt in each year was quite low, ranging from 0.11 to 0.13 in the second and third years.

These findings suggest that individuals with literacy or language problems might benefit from services designed to increase their earnings potential, but the findings also indicate that these individuals do not appear to impose a major burden on the UI system. In fact, the opposite seems true; individuals in this group received fewer UI benefits after the initial benefit year than did other demonstration group members. This result may be due to the fact that members of the subgroup with literacy and language problems had low post-UI earnings, or it may be due to their movement out of the New Jersey covered sector. Unfortunately, we cannot distinguish between these two possibilities.

TABLE III.10

QUARTERLY EARNINGS MINUS AVERAGE QUARTERLY BASE-PERIOD EARNINGS FOR INDIVIDUALS WITH EARNINGS IN THE LITERACY AND LANGUAGE PROBLEM SUBGROUP

(Standard Deviations in Parentheses)

Quarter After Initial Claim	JSA-Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus
Base Period (Average per Quarter)	4,384 (2,802)	4,726 (3,439)	4,737 (3,384)
1	-2,014 (3,580)	-2,253 (3,020)	-1,617 (3,824)
2	-1,050 (3,236)	-1,071 (2,636)	-964 (2,983)
3	-924 (2,914)	-1,131 (3,133)	-601 (3,276)
4	-804 (2,717)	-851 (2,594)	389 (2,566)
5	-603 (2,586)	-537 (2,449)	-289 (2,777)
6	-421 (2,709)	-578 (2,534)	-283 (2,822)
7	-327 (2,504)	-237 (2,455)	-123 (2,943)
8	-414 (2,659)	-311 (2,440)	210 (3,015)
9	-113 (2,528)	-214 (3,047)	119 (3,042)
10	66 (2,635)	-220 (2,988)	379 (3,126)
Number of Observations	230	377	217

TABLE III.11

UI RECEIPT BY THE LITERACY AND LANGUAGE PROBLEM SUBGROUP
(Standard Deviations in Parentheses)

	JSA-Only			JSA Plus Training or Relocation			JSA Plus Reemployment Bonus		
	Benefits (Dollars)	Weeks Paid	Probability of Receipt	Benefits (Dollars)	Weeks Paid	Probability of Receipt	Benefits (Dollars)	Weeks Paid	Probability of Receipt
Year of Initial Claim (First Year)	3,366 (1,761)	19 (8.4)	n.a.	3,462 (1,722)	20 (7.7)	n.a.	3,353 (1,795)	19 (8.4)	n.a.
Second Year	266 (929)	1.3 (4.1)	0.12 (0.33)	328 (1.081)	1.8 (5.6)	0.12 (0.33)	345 (1,091)	1.8 (5.5)	0.13 (0.34)
Third Year	323 (1,136)	1.7 (5.8)	0.11 (0.32)	320 (1,072)	1.9 (5.8)	0.11 (0.32)	321 (1,188)	1.7 (5.8)	0.11 (0.31)
Fourth Year	201 (812)	1.1 (4.2)	0.07 (0.26)	103 (543)	0.7 (3.3)	0.05 (0.22)	183 (833)	0.9 (4.1)	0.06 (0.24)
Total	4,156 (2,683)	23 (12.6)	n.a.	4,229 (2,404)	24 (12.3)	n.a.	4,202 (2,565)	24 (12.5)	n.a.
Total After Demonstration	790 (2,092)	4.1 (10.0)	n.a.	767 (1,767)	4.4 (10.0)	n.a.	849 (1,823)	4.4 (9.0)	n.a.
Number of Observations		230			377			217	

n.a. = not applicable.

C. ASSESSMENT OF IMPACTS BY TYPE OF CLAIMANT

As stated in previous chapters, the purpose of the NJUIRDP was to target reemployment services to claimants who were likely to be displaced and were likely to experience difficulty in becoming employed. In a previous report (Corson et al., 1989), we have shown that the eligible claimants who entered the demonstration and were not offered services (the control group) did have longer spells of UI receipt than did noneligible UI claimants, suggesting that the demonstration was successfully targeted to workers who faced potential difficulties in the labor market. In addition, we have demonstrated that the services provided in the intervention did reduce the amount of UI benefits received by claimants, both in the year of the initial claim and in the following year. However, to understand more fully how the services offered in the demonstration affected behavior it is useful to analyze the impacts of the treatments on different subgroups to determine the subgroups that were affected most by the services.[3]

Estimates of the impact of JSA-only services on UI benefits for various subgroups, which are reported in Table III.12, reveal only modest variation in impacts across subgroups in the year of the initial claim. None of the subgroup differences in the year of the initial claim, which are reported in the first column of Table III.12, was statistically significant at the 90 percent confidence level, and only one of the subgroup impacts--for claimants from non-durable manufacturing industries--differed significantly from zero.

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[3] We conducted the subgroup analysis by extending the regression model to include interaction terms in the regression equation. These interaction terms were equal to the product of the treatment indicators and the variables that described the characteristics of the claimant that we predicted would affect the size of the estimated impact. We used linear combinations of the appropriate estimated parameters from this regression to provide estimates of the impact by subgroups, and calculated significance tests among subgroups (for example, males and females) within individual categories of characteristics (for example, gender). Because some subgroups were defined with data from the follow-up interview conducted as part of the original demonstration, we used a subsample of the full sample for which we also had interview data to estimate the impacts of the treatment by subgroup.

TABLE III.12

IMPACTS ON UI BENEFITS PAID BY SUBGROUP:
JSA-ONLY
INTERVIEW SAMPLE
(Dollars)

Subgroup	Year of Initial Claim (First Year)	Second Year	Third Year	Fourth Year	Total Post-Demonstration
Gender					
Female	-127	-105	-17	42	-81
Male	-15	-57	62	-40	-41
Race					
White	7	20	71	20	104
Black	-231	-466 ***,###	-85	-116	-667 **,###
Other non-white	-235	-131	-69	34	-166
Age					
Ages 25 to 34	-18	91 ##	144	29	253 #
Ages 35 to 54	-91	-152 **	-23	-17	-193
55 or older	-94	-139	-28	6	-162
Education					
High school dropout	69	80	215 *	102	395 *
High school graduate	-101	-161 **,#	-33 #	-50 #	-245 *,##
College graduate	-172	-160	-182	44	-329
Recall Status					
Expected recall	-156	-152	-39	-32	-237
Did not expect recall	-29	-46	53	17	26
Occupation in Pre-UI Job					
White collar	-21	-41	71	-36	-15
Clerical	-149	-82	-138	35	-188
Blue-collar	-71	-101	51	9	-42
Industry in Pre-UI Job					
Durable manufacturing	81	-99	178	75	152
Nondurable manufacturing	-315 *	-98	-69	-79	-252
Nonmanufacturing	-47	-71	3	2	-70

NOTE: The subgroup impacts were estimated with the interview sample. The statistical tests show (1) which subgroup impacts differ significantly from zero at conventional levels (*), and (2) which of the subgroup impacts differs significantly within categories (#)--for example, impacts for males are compared with impacts for females. For characteristics with more than two subgroups, the tests are, respectively, comparisons with whites, ages 35 to 54, high school dropouts, blue-collar workers, and workers from non-manufacturing industries.

*The null hypothesis that the impact is zero is rejected at the 90 percent confidence level in a two-tail test.

**The null hypothesis that the impact is zero is rejected at the 95 percent confidence level in a two-tail test.

***The null hypothesis that the impact is zero is rejected at the 99 percent confidence level in a two-tail test.

#The null hypothesis that the impact for this subgroup is equal to the impact for the comparison subgroup (see note above) is rejected at the 90 percent confidence level.

##The null hypothesis that the impact for this subgroup is equal to the impact for the comparison subgroup (see note above) is rejected at the 95 percent confidence level.

###The null hypothesis that the impact for this subgroup is equal to the impact for the comparison subgroup (see note above) is rejected at the 99 percent confidence level.

On the other hand, the impacts for subgroups show more variation in the postdemonstration period, as shown in the remaining columns of Table III.12. The impacts of total UI benefits received over the last three years of the follow-up period are reported in the last column of Table III.12. Two of the subgroup differences in this postdemonstration period were statistically significant at the 90 percent level of confidence. The significant differences indicate that the JSA-only impacts on postdemonstration UI benefits were greater for middle-age claimants than for younger claimants, and the impacts were greater for high school graduates than for other claimants. Throughout the postdemonstration period, the point estimates of the UI impacts were also somewhat greater for some subgroups than for others. Specifically, estimated impacts were greater for claimants who expected to be recalled than for those who did not, greater for workers from nondurable manufacturing industries than for workers from durable manufacturing industries, greater for clerical workers than for other white-collar workers or blue-collar workers, and greater for women than for men. However, these differences were not statistically significant over the entire postdemonstration period. Finally, impacts were significantly greater for black claimants than for white claimants.

The lack of statistical significance in most of these differences makes it difficult to draw useful conclusions from the JSA-only impacts on UI receipt by subgroups. However, the subgroup estimates provide little evidence that the JSA services were more effective at reducing UI benefits for individuals who faced hard-core, structural unemployment problems than for individuals with readily marketable skills. In fact, it appears that individuals who would be expected to have less trouble in finding reemployment (such as high school graduates) may have been affected by the treatment to a greater extent than were individuals who might more accurately be called displaced workers (such as workers from durable-goods industries, which have declined steadily in New Jersey in recent years, and permanently separated workers). That is, displaced workers with more severe reemployment difficulties may have been affected less by this treatment than were other workers who faced more favorable reemployment prospects.

Such a conclusion is consistent with the design of the JSA-only treatment, which was intended to help claimants recognize their useful work skills and market them effectively, and to encourage them to search actively for reemployment by requiring that they maintain contact with the demonstration office and engage in job-search activities. Because the JSA-only treatment was not designed to enhance the skills of claimants directly, it had less potential to affect the employment prospects of workers whose skills were obsolete or who faced other significant barriers to reemployment.

In contrast, the JSA plus training or relocation treatment was designed to retrain workers whose skills were obsolete or who faced other barriers to reemployment. Despite this design, the impacts of the JSA plus training or relocation treatment appear to have been greatest for the same subgroups that were affected to the greatest extent by the JSA-only treatment, as shown in Table III.13. As we argued earlier, these subgroups include claimants who are not usually thought to be dislocated. For example, impacts during the year of the initial claim were significantly greater for claimants who expected to be recalled than for those who did not. In the postdemonstration period, the impacts were greater for high school and college graduates than for dropouts, greater for workers from nondurable manufacturing than for workers from durable manufacturing, and greater for clerical workers than for blue-collar workers. These findings support the argument that the demonstration had a larger impact on claimants who appear to have had relatively marketable skills.

In the year after the initial claim, the JSA plus reemployment bonus treatment also had a greater impact on UI receipt for claimants with readily marketable skills (Table III.14). For example, the impact of the treatment on benefits received by white-collar workers and clerical workers was significantly greater than the impact on the benefits received by blue-collar workers

TABLE III.13

IMPACTS ON UI BENEFITS PAID BY SUBGROUP: JSA PLUS TRAINING OR RELOCATION INTERVIEW SAMPLE
(Dollars)

Subgroup	Year of Initial Claim (First Year)	Second Year	Third Year	Fourth Year	Total Post-Demonstration
Gender					
Female	-49	-59	17	65	16
Male	-106	-78	2	20	-59
Race					
White	-20	-8	73	45	104
Black	-278 *	-215 *	-168 #	-5	-390 *,#
Other non-white	-120	-179	-80	75	-185
Age					
Ages 25 to 34	-84	60	94	119	261
Ages 35 to 54	-100	-77	0	43	-32
55 or older	-24	-201 **	-70	-46	-328 *
Education					
High school dropout	-70	26	148	113	273
High school graduate	-106	-84	-74 #	8	-148 #
College graduate	79	-291 *	29	74	-219
Recall Status					
Expected recall	-273 **,##	-78	129	65	109
Did not expect recall	19	-64	-50	31	-86
Occupation in Pre-UI Job					
White collar	-30	4	-5	107	97
Clerical	-208	-234 **	-140	-16	-390 *
Blue-collar	-58	-51	67	29	40
Industry in Pre-UI Job					
Durable manufacturing	-16	-59	106	112	158
Nondurable manufacturing	-46	-163	-225 *,#	-95	-479 **,##
Nonmanufacturing	-104	-45	47	61	54

NOTE: The subgroup impacts were estimated with the interview sample. The statistical tests show (1) which subgroup impacts differ significantly from zero at conventional levels (*), and (2) which of the subgroup impacts differs significantly within categories (#)--for example, impacts for males are compared with impacts for females. For characteristics with more than two subgroups, the tests are, respectively, comparisons with whites, ages 35 to 54, high school dropouts, blue-collar workers, and workers from non-manufacturing industries.

[*] The null hypothesis that the impact is zero is rejected at the 90 percent confidence level in a two-tail test.

[**] The null hypothesis that the impact is zero is rejected at the 95 percent confidence level in a two-tail test.

[***] The null hypothesis that the impact is zero is rejected at the 99 percent confidence level in a two-tail test.

[#] The null hypothesis that the impact for this subgroup is equal to the impact for the comparison subgroup (see note above) is rejected at the 90 percent confidence level.

[##] The null hypothesis that the impact for this subgroup is equal to the impact for the comparison subgroup (see note above) is rejected at the 95 percent confidence level.

[###] The null hypothesis that the impact for this subgroup is equal to the impact for the comparison subgroup (see note above) is rejected at the 99 percent confidence level.

TABLE III.14

IMPACTS ON UI BENEFITS PAID BY SUBGROUP: JSA PLUS REEMPLOYMENT BONUS INTERVIEW SAMPLE (Dollars)

Subgroup	Year of Initial Claim (First Year)	Second Year	Third Year	Fourth Year	Total Post-Demonstration
Gender					
Female	-155	-91	-48	32	-114
Male	-118	-44	-13	15	-46
Race					
White	-121	-29	33	64	60
Black	-237	-156	-319 **,##	-152 *,##	-628 **,##
Other Nonwhite	-105	-140	-23	24	-140
Age					
Ages 25 to 34	52 ##	-81	70	67	48
Ages 35 to 54	-308 ***	-79	-136 *	22	-193
55 or older	-13 #	-30	62	-24	-5
Education					
High school dropout	15	-202 *	-50	-153 **	-113
High school graduate	-239 ***	-1	-62	-42 ##	-103
College graduate	52	-189	74	74	-69
Recall Status					
Expected recall	-307 ***,#	-89	-38	6	-128
Did not expect recall	-52	-57	-27	32	-57
Occupation in Pre-UI Job					
White collar	-298 **,#	-108	-21	-43	-179
Clerical	-360 **,#	-255 **,#	-243 *	2	-495 **,#
Blue-collar	20	15	35	64	108
Industry in Pre-UI Job					
Durable manufacturing	-131	-131	-43	29	-144
Nondurable manufacturing	-350 **	-78	-124	52	-149
Nonmanufacturing	-78	-47	-1	14	-42

NOTE: The subgroup impacts were estimated with the interview sample. The statistical tests show (1) which subgroup impacts differ significantly from zero at conventional levels (*), and (2) which of the subgroup impacts differs significantly within categories (#)--for example, impacts for males are compared with impacts for females. For characteristics with more than two subgroups, the tests are, respectively, comparisons with whites, ages 35 to 54, high school dropouts, blue-collar workers, and workers from nonmanufacturing industries.

[*] The null hypothesis that the impact is zero is rejected at the 90 percent confidence level in a two-tail test.

[**] The null hypothesis that the impact is zero is rejected at the 95 percent confidence level in a two-tail test.

[***] The null hypothesis that the impact is zero is rejected at the 99 percent confidence level in a two-tail test.

[#] The null hypothesis that the impact for this subgroup is equal to the impact for the comparison subgroup (see note above) is rejected at the 90 percent confidence level.

[##] The null hypothesis that the impact for this subgroup is equal to the impact for the comparison subgroup (see note above) is rejected at the 95 percent confidence level.

[###] The null hypothesis that the impact for this subgroup is equal to the impact for the comparison subgroup (see note above) is rejected at the 99 percent confidence level.

at the 90 percent confidence level. White-collar and clerical workers faced greater reemployment opportunities than blue-collar workers during the demonstration, due to the substantial growth in service industries in New Jersey over the period of the demonstration. Impacts were also significantly greater (at the 90 percent confidence level) for claimants between the ages of 35 and 54 than for younger or older claimants.

Given the theory underlying the use of a reemployment bonus, it is also not surprising that the JSA plus reemployment bonus treatment had its greatest impact on claimants who had marketable skills in the short-term. The purpose of a reemployment bonus is to create a greater financial incentive to become reemployed rapidly; it has no impact on the skills of claimants. Therefore, claimants who already had marketable skills were best equipped to find a job quickly and to take advantage of the bonus offer.

After the year of the initial claim, some of the long-term impacts of the JSA plus reemployment bonus treatment on UI receipt by subgroup differed from the short-term impacts. In the first year after the initial claim year, UI impacts were significantly larger for high school dropouts and college graduates than for high school graduates. These differences reversed themselves in the third year after the initial claim year, in which UI impacts were significantly smaller for high school dropouts and college graduates than for high school graduates. While the impact of UI receipt for white-collar workers was high in the year of the initial claim, the impact for these workers was relatively small in following years. These findings suggest that some of the subgroups that were not significantly affected by the JSA plus reemployment bonus treatment in the short-term were significantly affected in the long-term.

The evidence on the short-term and long-term UI impacts by subgroups suggests that the strategy of targeting services to displaced workers may not maximize the impact of the services used in the NJUIRDP on UI receipt. All three treatments appear to have had their greatest impact on workers who had readily marketable skills and experience. But the eligibility requirements of the demonstration specifically attempted to target services to individuals who, in the absence of services, were expected to have difficulty in finding reemployment. Given the subgroup findings, the demonstration might have had an even greater impact on UI receipt if the eligibility requirements had been set whereby a wider range of claimants were enrolled, including those whose reemployment prospects were relatively good.

IV. BENEFIT-COST ANALYSIS

In this chapter, we combine estimates of the impacts of the NJUIRDP presented in earlier chapters with estimates of the net costs of the demonstration to assess whether, compared with the existing UI system, the benefits of each treatment exceeded its costs. We also assess benefits and costs from several other perspectives--those of the major groups affected by the demonstration policies (claimants, employers, and the government budget) and the perspective of society as a whole. The purpose of this exercise is to summarize the information from the evaluation in order to help policymakers determine the relative desirability of providing any of these treatments on an ongoing basis. Our benefit-cost evaluation addresses several issues:

- The costs of providing each of the three treatments on an ongoing basis relative to the costs of existing services (these are referred to as net costs)
- The effects of each treatment compared with existing services from the perspectives of society as a whole, claimants, employers, and the government (these are referred to as net effects)--that is, whether benefits outweigh costs or vice versa
- Whether the offer of training and relocation assistance or the offer of the reemployment bonus generated benefits that exceeded the costs of these additional services
- How the benefits and costs of the treatments are allocated among Department of Labor programs (that is, UI, ES, and JTPA programs at the local, state, and federal levels) and the rest of the government sector

In the initial evaluation we presented the results of a benefit-cost analysis of the NJUIRDP based on the impacts measured during the year of the initial UI claim. The results of that earlier analysis showed that all three of the treatments offered net benefits both to society as a whole and to claimants when compared with existing services. The JSA-only and JSA plus reemployment bonus treatments also led to net gains to the government sector as a whole, although none of the treatments led to net benefits to the Labor Department agencies which actually offered the services.

In this report, we extend the benefit-cost analysis by incorporating the longer-term impacts described in previous chapters of this report. Since the longer-term impacts provided evidence that the treatments generated additional UI savings, the results of the benefit-cost analysis are more favorable than they were in the initial evaluation.[1] We use two approaches to incorporate the long-term impacts into the benefit-cost analysis. Our basic approach is to use only the long-term impacts on UI receipt to extend the analysis, since the impacts on UI receipt were the only long-term impacts that were found to be statistically significant. In this approach, as in Corson et al. (1989), we use the estimated impacts on earnings based on the follow-up interview in the analysis, thereby assuming implicitly that all impacts on earnings took place before the follow-up interview. We also modify the basic approach by incorporating into the analysis the estimates of the long-term impacts on earnings that were presented in previous chapters. These earnings impacts were based on data from quarterly wage records. Because we have wage records for 10 quarters following the period of the demonstration, we are able to allow for the possibility that the treatments may have affected the earnings of claimants over this entire period.

We explore these issues by using a comprehensive benefit-cost analytical framework.[2] In Section A we discuss this approach to benefit-cost analysis, and describe how the benefits and costs were calculated. In Section B, we present the results of our benefit-cost analysis of the three treatments. Section C contains a summary and conclusions.

A. METHODOLOGY

The comprehensive accounting framework that we use to compare the benefits and costs of the three treatments in the NJUIRDP includes several steps. The first step is to define the various perspectives from which benefits and costs are measured. We consider the benefits and costs to the UI claimants themselves, to determine whether the treatments were beneficial to those whom they were designed to serve. We also consider the perspective of the employers who hired the claimants, to examine the net effects of their hiring decisions, and we consider the perspective of the government, to assess the budgetary impacts of each treatment relative to existing programs. We also break the government perspective down into Labor Department programs (that is, UI, ES, and JTPA) versus other government programs, to obtain more specific insight into the budgetary implication of these treatments.

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[1] The decrease in UI payments to the treatment group also causes a slight decrease in tax payments because the decrease in benefits lowers claimants' incomes.

[2] See Long, Mallar, and Thornton (1981) for one of the initial applications of this framework.

After the relevant perspectives have been defined, the next step in the analysis is to construct a comprehensive list of the expected benefits and costs from each perspective.[3] From the perspective of UI claimants, the key benefit of the demonstration treatments would be an increase in earnings and fringe benefits generated by more rapid reemployment (and/or higher earnings). More rapid reemployment should also be a psychological benefit to claimants, since most persons find unemployment stressful. On the other hand, the increased reporting requirements under the demonstration imposed a cost on claimants by reducing their time for leisure and non-market activities. Other costs to claimants are the loss of some UI benefits from more rapid reemployment, the additional taxes that they must pay on their increased earnings, and any costs from working (for example, child care or transportation expenses).

Employers benefit from the increased output produced by claimants who are hired more rapidly due to the treatment, but they also incur costs because they must compensate these employees with salaries and fringe benefits. We make the assumption that the value of the additional output to employers equals the value of the additional compensation by employers, which implies that they will incur no net benefits or costs from these treatments. However, this assumption may understate the benefits derived by employers from a labor market that functions more effectively, which would reduce their recruiting and turnover costs. Alternatively, the treatments might impose a cost on some employers if some temporarily laid-off workers were unavailable for rehire.[4] The Labor Department perspective includes the perspectives of UI and ES, whose operations are funded through the UI payroll tax, and the perspective of the JTPA system, which is funded through general revenues. These agencies would incur the costs of providing each of the three treatments in an ongoing program. They would benefit from their direct share of tax increases paid by claimants and their employers, and from reductions in UI benefits paid to claimants and in the costs of providing those benefits. In addition, their costs would be partially offset by a reduction in the costs of providing existing services. Whether the increase in payroll taxes or the reduction in UI benefits was large enough to offset the net costs of the demonstration was one of the key issues in this analysis.

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[3] Some of the benefits and costs are difficult to value in dollar terms. Although these "intangible" benefits (or costs) are not susceptible to measurement (such as the psychological benefits to claimants from obtaining a job), it is still important that they be assigned to the specific perspective so that policy judgments can be made about their likelihood of affecting the measured benefit-cost comparisons.

The other sectors of government inevitably derive net benefits from these treatments (assuming that at least some positive earnings impacts occur) because they receive the portion of claimants' taxes not used to fund Labor Department programs.[5]

The benefits and costs from all of these perspectives are summed to determine the benefits and costs to society as a whole. On the benefit side, the increased earnings of claimants are considered to represent an increase in total output and thus a net benefit to society. The assumption underlying this approach to valuing output is that the more rapid reemployment of demonstration claimants did not displace the employment of other individuals. This no-displacement assumption seems reasonable given the strength of the New Jersey economy. On the cost side, the net operational costs of offering each treatment represent social resources that could be spent otherwise, and are thus measured as net costs to society.

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[4] Both employers and claimants could also be affected by any changes in taxes due to an increase (or reduction) in government costs from offering the treatments. However, any such changes would occur only in the long run, and their effect would depend on how the treatments were funded. For this reason, they are not included in this analysis.

[5] Other sectors of the government could also benefit if the treatments reduced the receipt of such benefits as food stamps or other public assistance. This potential effect was examined, but since no impacts were found we have not included these potential impacts in the benefit-cost framework.

The general approach for valuing the benefits and costs of the three treatments is to measure the market value of the resources consumed, saved, or produced due to the treatment, compared with the existing services available to UI claimants. The market value of these resources are estimated for the period in which they were expended or received. We estimated the costs of the demonstration for the time period during which the demonstration operated (which corresponded roughly to fiscal year 1987), based on the assumption that all costs were incurred during this period. On the other hand, it is possible that the benefits of the demonstration were realized over a longer period of time. In our benefit-cost analysis, we allowed for the potential impact of the demonstration on UI benefits and earnings in the years following the demonstration. All these long-term impacts are expressed in terms of their present value during the operational period of the demonstration.[6] The impacts on UI benefits in years following the year of the initial claim were deflated using the GNP implicit price deflator and discounted using a 5 percent discount rate. Impacts on earnings that occurred after the year of the initial claim were deflated and discounted in the same way as the impacts on UI benefits.

For the purposes of the benefit-cost analysis, we wish to measure the net cost of each of the three treatments--using only those costs that would be incurred in an ongoing program--relative to the costs of the UI, ES, and JTPA services that are currently used by the target population. This comparison is based on the principle that claimants in the demonstration treatments received some services that they would have received even in the absence of the demonstration. For example, some claimants whom the demonstration referred to JTPA services would have gone to

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[6] Actual market prices are used to value benefits and costs whenever available, on the assumption that these prices are the best measure of the true costs of these resources. When market prices are not available, the dollar value of resources must be estimated. For example, we estimated the value of fringe benefits, taxes, and the administrative costs of government agencies.

JTPA for services on their own. In order to measure the extent to which the costs of the demonstration services were greater than the costs of providing the existing services, we compared the costs of the demonstration services with the costs of the services received by the control group. Corson et al. (1989) describe the calculations of the costs of the services provided in the demonstration.

B. BENEFITS AND COSTS FROM ALTERNATIVE PERSPECTIVES

In this section, we present estimates of the net benefits and costs of the three treatments relative to the existing services available to UI recipients. As discussed previously, our basic approach in this analysis relies on administrative data on long-term UI receipt and interview data on earnings. We then extend our basic approach by using long-term earnings impacts based on the wage records.

1. Benefit-Cost Analysis of JSA-Only versus Existing Services

The benefits of the JSA-only treatment outweighed the costs from the perspectives of claimants, the government, and society as a whole, while the benefits exactly offset the costs for the Labor Department. Our estimates, which are summarized in Table IV.1, reveal that members of the JSA-only group increased their earnings by an average of $608 relative to members of the control group, and we imputed another $128 in additional fringe benefits to reach a total increase of $736 in compensation. Much of this increase in compensation benefitted the claimants, but enough of the increased earnings was returned to the government sector via increased taxes and reduced UI benefits that the government realized a net gain of $134 per claimant. When we examined the UI, ES, and JTPA programs that comprise the Labor Department sector, we found that the savings were just equal to the costs of the JSA-only treatment. The estimated net social gain, which can be taken as an indicator of the efficiency of the treatment, is $581 per claimant.

TABLE IV.1

BENEFIT-COST COMPARISON OF THE JSA-ONLY TREATMENT WITH EXISTING SERVICES
(Dollars per Claimant)

				Government		
Benefits and Costs	Society	Employers	Claimants	Labor Dept.	Other Government	Government Total
Market Output and Wages						
Increased output	736	736	0	0	0	0
Wages and fringe benefits	0	-736	736	0	0	0
Tax Payments						
Claimants' taxes	0	0	-141	6	135	141
Income Support Payments						
UI payments	0	0	-148	148	0	148
Other payments	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI payment administration	1	0	0	1	0	1
Administration of other programs	0	0	0	0	0	0
Demonstration Costs						
Classroom training costs	0	0	0	0	0	0
OJT costs	0	0	0	0	0	0
Relocation assistance	0	0	0	0	0	0
Reemployment bonuses	0	0	0	0	0	0
Local office labor costs	-118	0	0	-118	0	-118
Central office labor costs	-25	0	0	-25	0	-25
Other costs (direct and indirect)	-26	0	0	-26	0	-26
Offsetting Costs of Existing Services						
ES costs	10	0	0	10	0	10
JTPA costs	-1	0	0	-1	0	-1
UI costs	5	0	0	5	0	5
Sum of Measured Benefits and Costs	581	0	447	0	134	134
Nonmonetary Factors						
Psychological benefits of earlier reemployment	+		+			
Burden of reporting requirements, reduced leisure time, and costs from working	-		-			

NOTE: Row or column sums may not add to the totals due to rounding. UI payments were measured over a 3 to 4 year period following the claim date for each individual. Payments which were received in the years following the year of the initial claim were deflated using the GNP implicit price deflator and expressed as a present value (as of the year of the initial claim) based on a discount rate of 5 percent. All other outcomes presented in this table were measured during the year of the initial claim.

2. Benefit-Cost Analysis of the JSA plus Training or Relocation Treatment versus Existing Services

The JSA plus training treatment provided net benefits to claimants and generated net costs to the government sector, while society as a whole roughly broke even. The earnings of the JSA plus training claimants were $345 higher on average than those of control group members. We imputed an additional $72 of increased fringe benefits, for an increase of $417 in total compensation, as shown in Table IV.2. These increased earnings and fringe benefits represent a benefit to claimants, which was partially offset by an increase in taxes of $74 per person and a reduction in UI benefits of $111 per person. The estimated net benefit per claimant of the JSA plus training treatment (compared with existing programs) was $232.

The substantial costs of providing training to the JSA plus training group members who pursued this option, together with the prolonged UI benefits received by trainees while they participated in training, meant that the government sector incurred net costs for the JSA plus training treatment. Members of the treatment group were three to four times more likely to pursue training than were control group members. In addition, they received the same set of initial job-search services from the ES as did those in JSA-only treatment group. The increased taxes and reduced UI benefits that were generated by the increased employment of persons in the JSA plus training treatment substantially offset the costs of the JSA services, but did not begin to cover the costs of the training itself. The net government loss of $192 per claimant can be broken down into a $262 loss for Labor Department programs, and a $70 gain for the rest of the government.

From the perspective of society as a whole, the choice between the JSA plus training treatment and existing services appears fairly even, with a slight net benefit of $41 per person in favor of the treatment.

Compared with the JSA-only treatment, the benefits from the JSA plus training or relocation treatment were lower than their costs from all perspectives. In particular, JSA plus training or

TABLE IV.2

BENEFIT-COST COMPARISON OF THE JSA PLUS TRAINING OR RELOCATION ASSISTANCE TREATMENT WITH EXISTING SERVICES
(Dollars per Claimant)

				Government		
Benefits and Costs	Society	Employers	Claimants	Labor Dept.	Other Government	Government Total
Market Output and Wages						
Increased output	417	417	0	0	0	0
Wages and fringe benefits	0	-417	417	0	0	0
Tax Payments						
Claimants' taxes	0	0	-74	3	70	74
Income Support Payments						
UI payments	0	0	-111	111	0	111
Other payments	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI payment administration	1	0	0	1	0	1
Administration of other programs	0	0	0	0	0	0
Demonstration Costs						
Classroom training costs	-224	0	0	-224	0	-224
OJT costs	-23	0	0	-23	0	-23
Relocation assistance	-3	0	0	-3	0	-3
Reemployment bonuses	0	0	0	0	0	0
Local office labor costs	-183	0	0	-183	0	-183
Central office labor costs	-29	0	0	-29	0	-29
Other costs (direct and indirect)	-29	0	0	-29	0	-29
Offsetting Costs of Existing Services						
ES costs	10	0	0	10	0	10
JTPA costs	99	0	0	99	0	99
UI costs	5	0	0	5	0	5
Sum of Measured Benefits and Costs	41	0	232	-262	70	-192
Nonmonetary Factors						
Psychological benefits of earlier reemployment	+		+			
Burden of reporting requirements, reduced leisure time, and costs from working	-		-			

NOTE: Row or column sums may not add to the totals due to rounding. UI payments were measured over a 3 to 4 year period following the claim date for each individual. Payments which were received in the years following the year of the initial claim were deflated using the GNP implicit price deflator and expressed as a present value (as of the year of the initial claim) based on a discount rate of 5 percent. All other outcomes presented in this table were measured during the year of the initial claim.

relocation claimants realized lower earnings increases on average, but the costs of the treatment were much higher. It seems that the most plausible interpretation of this finding is that claimants in the JSA plus training treatment who entered training (or who hoped to enter training) deferred reentering the labor market, and did not increase their earnings capacity sufficiently to compensate for the fewer number of weeks that they worked in the year after the claim date.

3. Benefit-Cost Analysis of the JSA plus Bonus Treatment versus Existing Services

Members of the JSA plus reemployment bonus treatment group experienced earnings gains that were similar to those of claimants in the JSA-only group and larger UI benefit reductions than did the other two treatment groups, while the costs of the treatment fell in between the costs of the other two treatments. On balance, a substantial net gain of $565 per claimant accrued to society relative to existing services, as shown in Table IV.3.

Claimants experienced a net benefit of $427 on average, comprising a $591 increase in earnings and a $124 increase in fringe benefits, balanced by a $279 reduction in UI benefits and a $135 increase in taxes. The government benefitted overall from the treatment, and the Labor Department programs experienced a slight net gain of $9 per claimant. The rest of the government experienced a net gain of $154 from an increase in taxes.

Overall, the findings for the JSA plus reemployment bonus treatment were similar to those for the JSA-only treatment. The earnings gains experienced by claimants were similar, and while the bonus payments represented a cost to the government sector and a gain to claimants, this cost (and gain) was offset by the larger reduction in UI payments plus the increase in taxes relative to the JSA-only treatment. The results in Tables IV.1 and IV.3 show that the government received slightly higher net benefits from the JSA plus reemployment bonus treatment ($138 per claimant) than from the JSA-only treatment ($134 per claimant), although this difference is so small that no distinction can be made between the two treatments.

TABLE IV.3

BENEFIT-COST COMPARISON OF THE JSA PLUS REEMPLOYMENT BONUS TREATMENT WITH EXISTING SERVICES
(Dollars per Claimant)

Benefits and Costs	Society	Employers	Claimants	Government		
				Labor Dept.	Other Government	Government Total
Market Output and Wages						
Increased output	715	715	0	0	0	0
Wages and fringe benefits	0	-715	715	0	0	0
Tax Payments						
Claimants' taxes	0	0	-135	6	129	135
Income Support Payments						
UI payments	0	0	-279	279	0	279
Other payments	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI payment administration	1	0	0	1	0	1
Administration of other programs	0	0	0	0	0	0
Demonstration Costs						
Classroom training costs	0	0	0	0	0	0
OJT costs	0	0	0	0	0	0
Relocation assistance	0	0	0	0	0	0
Reemployment bonuses	0	0	125	-125	0	-125
Local office labor costs	-118	0	0	-118	0	-118
Central office labor costs	-30	0	0	-30	0	-30
Other costs (direct and indirect)	-26	0	0	-26	0	-26
Offsetting Costs of Existing Services						
ES costs	10	0	0	10	0	10
JTPA costs	8	0	0	8	0	8
UI costs	5	0	0	5	0	5
Sum of Measured Benefits and Costs	565	0	427	9	129	138
Nonmonetary Factors						
Psychological benefits of earlier reemployment	+		+			
Burden of reporting requirements, reduced leisure time, and costs from working	-		-			

NOTE: Row or column sums may not add to the totals due to rounding. UI payments were measured over a 3 to 4 year period following the claim date for each individual. Payments which were received in the years following the year of the initial claim were deflated using the GNP implicit price deflator and expressed as a present value (as of the year of the initial claim) based on a discount rate of 5 percent. All other outcomes presented in this table were measured during the year of the initial claim.

4. Benefit-Cost Analysis Based on Alternative Estimates of Earnings Impacts

In this section we recalculate the benefit-cost estimates using earnings impacts based on wage records rather than on the follow-up interviews. The wage-records data may understate earnings impacts because (1) the data do not include the earnings of the self-employed and those who find new employment outside New Jersey, and (2) the data begin with the first calendar quarter after the claim date, and thus miss treatment-control differences for much of the sample in the first month or two after the claim date. On the other hand, if the treatments do have long-term impacts on earnings, we can use the wage-records data to incorporate these long-term impacts into the benefit-cost analysis. The calculations of benefits and costs presented in this section are based on wage records for the first 10 calendar quarters following the claim date. Table IV.4 presents the newly calculated net benefit estimates and the benchmark estimates for each treatment.

For all three treatments, using earnings estimates based on the wage records greatly reduces the estimated benefits to the claimants, the government, and society as a whole relative to the benchmark estimates. For the JSA-only and the JSA plus reemployment bonus treatments, the net benefits to the claimants, the government, and society remain positive, while these perspectives show a net loss for the JSA plus training or relocation treatment. As expected, the net benefits and costs from the Labor Department perspective scarcely change, because we are not changing the impacts of the treatment on UI benefits.

These findings indicate that, despite the fact that we have earnings data for a longer follow-up period when we use the wage records rather than the interview data, the estimated net benefits of all of the treatments are greater when the calculations are based on the interviews. The earnings impacts do not appear to have persisted enough to have increased the measured net benefits of the treatments significantly.

TABLE IV.4

SUM OF THE BENEFITS AND COSTS OF ALL TREATMENTS BASED ON ALTERNATIVE ESTIMATES OF EARNINGS IMPACTS

(Dollars Per Claimant)

			Government		
	Society	Claimants	Labor Dept.	Other Government	Government Total
JSA-Only Treatment					
Using earnings impacts from interview data[a]	581	447	0	135	134
Using earnings impacts from wage-records data[b]	478	368	-1	111	110
JSA Plus Training Treatment					
Using earnings impacts from interview data[a]	41	232	-262	70	-192
Using earnings impacts from wage-records data[b]	-348	-63	-266	-19	-285
JSA Plus Bonus Treatment					
Using earnings impacts from interview data[a]	565	427	9	129	138
Using earnings impacts from wage-records data[a]	285	214	6	65	71

[a] Earnings impacts based on interview data were measured over the year of the initial claim. All other measures used in these calculations are defined as in Tables IV.1-IV.3.

[b] Earning impacts based on wage records were measured over the first 10 full calendar quarters following the initial claim. All other measures used in these calculations are defined as in Table IV.1-IV.3.

C. SUMMARY AND CONCLUSIONS

The results of our benefit-cost analysis suggest that all three treatments offer net benefits to society as a whole and to claimants relative to existing services. The JSA-only treatment and the JSA plus reemployment bonus treatment also led to net gains to the government sector as a whole and to the Labor Department agencies. On the other hand, the JSA plus training or relocation treatment is expensive for the government sector.

When we compared the JSA plus training or relocation treatment with the JSA-only treatment, we found that JSA plus training or relocation costs were higher (or benefits were lower) than those for the JSA-only treatment from all viewpoints, since the costs of the service component of the JSA plus training or relocation treatment were higher and its earnings gains were substantially lower. The JSA plus training or relocation treatment would look better if earnings gains could be sustained over several years, but evidence from the wage records do not show any impact of the treatment on earnings following the period of the demonstration.

The net benefits and costs of the JSA plus reemployment bonus treatment appear to be similar to those of the JSA-only treatment from all perspectives, although the bonus generated higher costs from the government perspective. These findings suggest that the JSA-only and the JSA plus reemployment bonus treatments generate savings in UI benefits and increases in UI taxes that are greater than the cost of the treatments. Our estimates indicate that the JSA-only treatment would pay for itself from the perspective of the Labor Department, while the JSA plus reemployment bonus treatment would lead to modest net benefits for the Labor Department. On the other hand, the costs of the JSA plus training or relocation treatment exceed the savings in UI benefits and increased taxes that are generated by the treatment. Use of this treatment would require either reducing funding for other programs or increasing taxes, since it appears to create net costs to the government as a whole.

V. CONCLUSION

The New Jersey Unemployment Insurance Reemployment Demonstration tested alternative strategies to provide displaced workers with early intervention services to accelerate their return to work. Three treatments were tested in the demonstration: (1) job-search assistance only, (2) job-search assistance combined with training or relocation assistance, and (3) job-search assistance combined with a cash bonus for early reemployment. The initial evaluation of the demonstration (Corson et al., 1989) found that each of the treatments reduced UI collections and increased employment and earnings in the year following the initial UI claim. This follow-up study examines UI and earnings impacts over a three- to four-year period beginning with the initial UI claim.

A. JOB-SEARCH ASSISTANCE

Our estimates demonstrate that the JSA-only treatment reduced UI benefits both in the year of the initial claim and in the following year, while the estimated impacts in the third and fourth years were small and not statistically significant. Estimates based on wage records suggest that the JSA-only treatment had no significant impacts on the employment or earnings of claimants. However, an analysis of earnings outcomes based on interview data suggests that the JSA-only treatment increased earnings in the short-run. When the benefits of the treatment over the entire follow-up period were weighed against its costs, we found that the JSA-only treatment yielded net benefits to claimants, to the government, to the Labor Department agencies, and to society as a whole.

These findings strongly suggest that the JSA-only treatment reduced UI benefits paid to claimants, and that part of the impact occurred after the year of the initial claim. Although we observe the impacts on UI receipt caused by the JSA-only treatment, we cannot determine which components of the JSA-only treatment were instrumental in bringing about this impact. The job-search assistance treatment was designed both to help claimants improve their job-finding skills and to encourage claimants to search actively for work. To this end, the treatment provided claimants with additional services related to job-search activities and required that they maintain periodic contact with the demonstration office to discuss their job search. Given the design of the treatment, we cannot separate the impact of the job-search services from the impact of the reporting requirements. Consequently, the estimated impact of the treatment is a combination of the impact of the enhanced services and the impact of the reporting requirements.

In addition, the JSA-only treatment does not appear to have been more effective at reducing UI benefits for individuals who faced hard-core, structural unemployment problems than for individuals with readily marketable skills. In fact, the treatment appears to have affected individuals who faced fewer barriers to reemployment to a greater extent than it did individuals who faced extensive barriers to reemployment. Such a conclusion is consistent with the design of the JSA-only treatment, which was intended to help claimants recognize their existing work skills and market them effectively rather than to help them learn new work skills. Given these findings, the demonstration may have had an even greater impact on UI receipt if the eligibility requirements had been set whereby a wider range of claimants were enrolled, including those with relatively good employment prospects.

B. JOB SEARCH ASSISTANCE PLUS TRAINING OR RELOCATION ASSISTANCE

The impacts of the JSA plus training or relocation assistance treatment on UI receipt, employment, and earnings were similar to the impacts of the JSA-only treatment in all periods. The only exception occurred in the second year after the initial claim, in which the impact of the JSA plus training or relocation treatment was substantially smaller than the impact of the JSA-only treatment and not statistically significant. Like the JSA-only treatment, the JSA plus training treatment clearly yielded net benefits for UI claimants. However, due to the high costs of providing training, the treatment was costly from the government and Labor Department perspectives and yielded only minimal net benefits for society as a whole, even after allowing for the possibility that training had an impact on long-term earnings or UI receipt.

While these findings suggest that the training component of the JSA plus training or relocation treatment had no additional impact on UI receipt or earnings beyond the impact of the JSA services, this conclusion may be misleading. Since only a relatively small percentage (15 percent) of claimants who were offered training participated in training, the impacts of training would need to be quite large to be detected in our formal estimates of treatment impacts. A further examination of employment and earnings data for trainees suggests that training did have a positive impact on the employment and earnings of trainees in the long-run. The results were especially strong for on-the-job trainees, whose training significantly increased their employment and earnings over the entire period of observation. There are also some indications that trainees received lower UI benefits in later years, but the differences were not statistically significant for most years.

C. JOB-SEARCH ASSISTANCE PLUS REEMPLOYMENT BONUS

The JSA plus reemployment bonus treatment had a larger impact on UI receipt than the other two treatments in both the short run and the long run, and it also increased employment and earnings significantly in the short run. Thus, the demonstration provides clear evidence that the offer of the reemployment bonus encourages workers to become reemployed more quickly and to collect less UI benefits than they would without the offer. However, even though the JSA plus bonus treatment had larger impacts on earnings and UI receipt than the JSA-only treatment, the two treatments yield almost identical net benefits from all perspectives due to the higher costs associated with the JSA plus reemployment bonus treatment.

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APPENDIX A

METHODOLOGICAL ISSUES AND SUPPLEMENTARY TABLES

In Chapter III of this report, we estimated the impact of training on earnings using an equation in which the dependent variable is the change in earnings between the base period and a given post-claim period. The approach is based on the assumption that the following equation determines earnings for an individual i at any point in time t:

(A.1) $Y_{it} = a_i + a_t + b_1X_{it},$

where Y_{it} is equal to the earnings of individual i at time t, X_{it} is a set of demographic variables that affect earnings, a_i is an individual-specific intercept, and a_t is a time-specific intercept.

Assume that individuals may or may not receive training before some period of time t + 1. If training is expected to have some impact on earnings, then the equation for earnings in period t + 1 takes the following form:

(A.2) $Y_{it+1} = a_i + a_{t+1} + b_1X_{it+1} + b_2T_{it+1} + b_3T_{it+1}{}^*X_{it+1},$

where T_{it+1} indicates whether individual i has received training at time t + 1, and the interaction term, $T_{it+1}{}^*X_{it+1}$, controls for the possibility that training has different impacts on persons with different demographic characteristics.

For our analysis, we will also assume that the values of the demographic variables do not change over time. This assumption is reasonable for most of the variables in X_{it}, such as the indicator for the sex of the claimant, which will clearly remain unchanged between t and t + 1. Based on this assumption, we can drop the time subscript from the X variables. After making this assumption, we can use equations (A.1) and (A.2) to yield an expression for the change in earnings between t and t+1 as:

(A.3) $Y_{it+1} - Y_{it} = c_{t+1} + b_2T_{it+1} + b_3T_{it+1}{}^*X_i,$

where $c_{t+1} = (a_{t+1} - a_t)$. Equation (A.3) does not contain any independent variables from X_i because they are assumed to remain constant over time, and thus cannot have an impact on the change in earnings over time, except as they interact with training. We use equation (A.3) as the basis for estimating the impact of training on earnings. Given this equation, the impact of training is equal to $b_2 + b_3X_i$. Tables A.1 and A.2 contain the estimated coefficients for calculating the impacts of training on employment and earnings in Chapter IV.

Ashenfelter (1978) and Card and Sullivan (1988) have demonstrated that a similar estimator provides a consistent estimate of the impact of training on the earnings of trainees if (1) shocks in earnings are uncorrelated with their own lagged values and with the decision to participate in training, and (2) the unobserved individual effects enter linearly into the earnings equation.[1] Given the design of the demonstration, we feel that the first assumption is reasonable for our analysis. Both trainees and nontrainees experienced shocks to their earnings levels that led them to apply for UI benefits. Therefore, training status is independent of the pre-claim level of earnings, because the base-period experience of trainees and nontrainees in the demonstration was similar. For the same reason, differences in post-training outcomes for trainees and nontrainees cannot be attributed to differences in the lagged values of earnings. Since the two groups of claimants experienced similar earnings shocks prior to their UI claim, some other factor must account for any differences in their post-claim earnings.

The second assumption is more stringent for our analysis. We specify a simple linear relationship between the unobserved individual effects, a_i, and the level of earnings in each period of time that allows us to ignore these effects when we base our regression analysis on the observed change in earnings over time. One can imagine, however, that some unobserved personal characteristic might affect an individual's ability to recover his or her previous level of earnings after a layoff. In this case,

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[1] Note that the specification of equation (A.2) is based on an additional assumption that the unobserved individual effects, a_i, do not interact with training when earnings are determined at $t + 1$.

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TABLE A.1

ESTIMATED EARNINGS REGRESSION USED
TO CALCULATE TRAINING IMPACTS

(Standard Errors in Parentheses)

Explanatory Variable	Dependent Variable									
	Quarter 1 Earnings Minus Base Period Earnings	Quarter 2 Earnings Minus Base Period Earnings	Quarter 3 Earnings Minus Base Period Earnings	Quarter 4 Earnings Minus Base Period Earnings	Quarter 5 Earnings Minus Base Period Earnings	Quarter 6 Earnings Minus Base Period Earnings	Quarter 7 Earnings Minus Base Period Earnings	Quarter 8 Earnings Minus Base Period Earnings	Quarter 9 Earnings Minus Base Period Earnings	Quarter 10 Earnings Minus Base Period Earnings
Intercept	-3,626 (89)	-3,053 (104)	-2,506 (92)	-2,436 (97)	-2,365 (94)	-2,205 (107)	-1,932 (116)	-2,015 (103)	-2,006 (102)	-1,918 (106)
On-the-Job Training	-523 (9,411)	3,058 (10,950)	2,099 (9,696)	-8,884 (10,281)	1,141 (9,882)	1,414 (11,349)	963 (12,304)	-854 (10,896)	7,351 (10,778)	98 (11,065)
Classroom Training	9,176 (2,389)	8,548 (2,780)	8,378 (2,461)	8,589 (2,905)	8,048 (2,511)	8,925 (2,881)	8,181 (3,122)	8,938 (2,792)	9,584 (2,736)	10,506 (2,814)
Interactions with Classroom Training:										
Quarter 1 cohort	815 (562)	330 (677)	242 (589)	-29 (634)	-554 (612)	1,240 (702)	-145 (760)	-294 (674)	-87 (688)	-706 (565)
Quarter 2 cohort	381 (570)	139 (663)	-230 (587)	-585 (621)	-1,336 (589)	-1,072 (687)	-621 (744)	-849 (659)	-759 (652)	-1,714 (671)
Quarter 3 cohort	1,294 (577)	133 (671)	437 (594)	143 (629)	-580 (606)	-214 (695)	-34 (734)	-48 (668)	-276 (660)	-1,105 (579)
Female	832 (420)	636 (489)	714 (433)	1,267 (456)	1,204 (442)	994 (507)	620 (549)	546 (487)	920 (481)	993 (485)
Black	2 (517)	230 (601)	455 (532)	123 (564)	14 (543)	-228 (623)	-257 (675)	-142 (566)	-97 (592)	-308 (609)
Other non-white	187 (650)	558 (757)	944 (670)	762 (709)	452 (684)	195 (764)	236 (650)	268 (753)	257 (745)	909 (766)
Age less than 35 years	371 (443)	631 (515)	404 (456)	490 (483)	439 (466)	375 (534)	337 (579)	180 (513)	713 (507)	427 (522)
Age 55 years or greater	-187 (556)	752 (647)	-441 (573)	-740 (606)	-1,113 (584)	-1,123 (670)	1,319 (727)	-987 (644)	-1,049 (637)	-1,302 (655)
Durable manufacturing	23 (518)	-447 (603)	-382 (534)	-264 (565)	327 (545)	462 (625)	167 (678)	120 (600)	138 (594)	-367 (611)
Nondurable manuracturing	217 (593)	-99 (690)	-231 (611)	-213 (647)	162 (624)	72 (716)	-12 (776)	-334 (687)	-213 (580)	-539 (699)
Union hiring	0 (1,053)	52 (1,225)	137 (1,084)	359 (1,148)	236 (1,106)	380 (1,269)	404 (1,376)	192 (1,219)	319 (1,208)	554 (1,240)

TABLE A.1 (continued)

	Dependent Variable									
Explanatory Variable	Quarter 1 Earnings Minus Base Period Earnings	Quarter 2 Earnings Minus Base Period Earnings	Quarter 3 Earnings Minus Base Period Earnings	Quarter 4 Earnings Minus Base Period Earnings	Quarter 5 Earnings Minus Base Period Earnings	Quarter 6 Earnings Minus Base Period Earnings	Quarter 7 Earnings Minus Base Period Earnings	Quarter 8 Earnings Minus Base Period Earnings	Quarter 9 Earnings Minus Base Period Earnings	Quarter 10 Earnings Minus Base Period Earnings
Expect recall	-306	257	635	115	310	329	645	849	763	663
	(537)	(625)	(554)	(566)	(565)	(648)	(702)	(622)	(615)	(633)
Potential duration	-182	-212	-200	-210	-211	-190	-179	-185	-228	-219
	(82)	(96)	(85)	(90)	(87)	(99)	(108)	(95)	(94)	(97)
Weekly benefit amount	-31	-27	-24	-20	-18	-19	-18	-18	-17	-18
	(5)	(6)	(5)	(6)	(6)	(6)	(7)	(6)	(6)	(6)
Paterson	-555	480	700	475	138	577	-408	-451	-698	-918
	(963)	(1,144)	(1,013)	(1,072)	(1,033)	(1,165)	(1,255)	(1,138)	(1,128)	(1,158)
Hackensack	-378	387	0	31	-718	-382	-663	-569	-1,006	-541
	(790)	(919)	(814)	(861)	(830)	(952)	(1,032)	(915)	(905)	(930)
Jersey City	-332	222	-382	-786	1,371	-961	-1,435	-1,599	1,453	85
	(978)	(1,138)	(1,008)	(1,067)	(1,028)	(1,160)	(1,278)	(1,133)	(1,120)	(1,152)
Butler	-700	70	24	46	67	-8	-697	-786	-1,700	-585
	(1,264)	(1,470)	(1,302)	(1,378)	(1,328)	(1,524)	(1,651)	(1,463)	(1,447)	(1,488)
Bloomfield	-986	-40	182	57	-354	-93	49	692	-295	-57
	(838)	(875)	(863)	(913)	(881)	(1,010)	(1,095)	(970)	(959)	(967)
Newark	-358	214	59	320	-7	72	-42	-151	-140	286
	(867)	(1,032)	(914)	(967)	(932)	(1,070)	(1,159)	(1,027)	(1,016)	(1,045)
Perth Amboy	-655	142	730	1,348	590	433	140	-81	-275	-643
	(913)	(1,062)	(940)	(995)	(959)	(1,101)	(1,193)	(1,057)	(1,045)	(1,075)
Burlington	-472	508	-501	-473	-1,267	-1,261	1,547	-1,456	-1,565	-1,193
	(802)	(934)	(827)	(875)	(844)	(968)	(1,049)	(929)	(919)	(945)
Deptford	1,168	1,099	349	-35	-604	-332	91	222	321	269
	(897)	(1,044)	(924)	(978)	(943)	(1,082)	(1,173)	(1,039)	(1,026)	(1,057)
Interactions with On-the-Job Training:										
Quarter 1 cohort	1,244	490	-2,285	-1,337	-1,589	-1,869	-1,527	-1,399	-5,307	-5,098
	(2,604)	(3,262)	(2,889)	(3,057)	(2,947)	(3,361)	(3,664)	(3,246)	(3,211)	(3,302)
Quarter 2 cohort	938	120	-1,069	-83	744	1,086	735	277	-1,402	-1,407
	(2,495)	(2,903)	(2,571)	(2,721)	(2,623)	(3,009)	(3,261)	(2,869)	(2,858)	(2,939)
Quarter 3 cohort	1,288	-48	-1,880	-1,323	-1,790	-2,361	-258	1,769	-1,113	-2,307
	(2,527)	(2,940)	(2,603)	(2,755)	(2,656)	(3,047)	(3,302)	(2,925)	(2,894)	(2,976)
Female	2,037	877	66	619	-536	-726	841	1,635	722	1,813
	(1,565)	(1,821)	(1,613)	(1,706)	(1,645)	(1,887)	(2,046)	(1,812)	(1,793)	(1,844)

TABLE A.1 (continued)

Explanatory Variable	Dependent Variable									
	Quarter 1 Earnings Minus Base Period Earnings	Quarter 2 Earnings Minus Base Period Earnings	Quarter 3 Earnings Minus Base Period Earnings	Quarter 4 Earnings Minus Base Period Earnings	Quarter 5 Earnings Minus Base Period Earnings	Quarter 6 Earnings Minus Base Period Earnings	Quarter 7 Earnings Minus Base Period Earnings	Quarter 8 Earnings Minus Base Period Earnings	Quarter 9 Earnings Minus Base Period Earnings	Quarter 10 Earnings Minus Base Period Earnings
Black	-444 (1,537)	-750 (1,789)	-348 (1,584)	531 (1,876)	-381 (1,616)	278 (1,854)	1,128 (2,009)	-355 (1,780)	-443 (1,761)	-383 (1,811)
Other non-white	-3,221 (2,822)	-2,576 (3,051)	-2,367 (2,701)	-1,343 (2,859)	-2,607 (2,756)	-1,616 (3,162)	-2,724 (3,427)	-2,502 (3,036)	-1,753 (3,003)	-1,836 (3,088)
Age less than 35 years	3,380 (1,462)	1,849 (1,724)	1,281 (1,520)	988 (1,815)	1,370 (1,557)	767 (1,787)	1,252 (1,936)	1,010 (1,715)	1,393 (1,697)	872 (1,745)
Age 55 years or greater		1,821 (2,822)	1,373 (2,321)	1,528 (2,457)	1,279 (2,368)	429 (2,717)	1,832 (2,945)	-291 (2,609)	137 (2,580)	611 (2654)
Durable manufacturing	1,150 (1,457)	789 (1,695)	-255 (1,501)	664 (1,586)	430 (1,531)	-272 (1,757)	722 (1,904)	681 (1,687)	1,178 (1,668)	240 (1,716)
Nondurable Manufacturing	-241 (3,014)	685 (3,507)	1,482 (3,105)	487 (3,286)	687 (3,168)	68 (3,634)	1,952 (3,939)	1,916 (3,489)	3,054 (3,452)	2,226 (3,550)
Expect recall	-1,518 (2,309)	177 (2,667)	-568 (2,379)	601 (2,517)	-1,174 (2,427)	-291 (2,784)	1,064 (3,018)	-745 (2,673)	724 (2,644)	1,446 (2,720)
Potential duration	-148 (219)	-155 (254)	-94 (225;	61 (238)	-95 (230)	-116 (264)	-129 (288)	-43 (253)	-116 (250)	-12 (258)
Weeklybenefit amount	-1 (18)	-2 (21)	4 (19)	15 (20)	4 (19)	6 (22)	11 (24)	22 (21)	-9 (21)	15 (22)
Paterson	4,219 (3,325)	3,766 (3,869)	4,191 (3,425)	5,425 (3,625)	4,496 (3,495)	3,925 (4,009)	3,585 (4,345)	1,391 (3,849)	1,402 (3,808)	1,799 (3,916)
Hackensack	3,133 (3,460)	2,148 (4,028)	3,008 (3,385)	3,818 (3,772)	2,749 (3,837)	2,000 (4,172)	2,332 (4,522)	130 (4,008)	7 (3,962)	800 (4,075)
Jersey City	864 (6,260)	-1,723 (6,121)	-725 (5,420)	-233 (5,735)	880 (5,526)	3 (6,343)	-2,508 (6,675)	-5,863 (6,080)	-2,171 (6,024)	-2 (6,198)
Butler	1,340 (4,045)	1,337 (4,707)	2,388 (4,168)	5,844 (4,410)	5,424 (4,252)	3,106 (4,878)	4,868 (5,287)	-1,207 (4,883)	5,001 (4,833)	4,148 (4,784)
Bloomfield	3,009 (3,569)	773 (4,178)	2,832 (3,897)	3,796 (3,813)	3,481 (3,772)	340 (4,327)	-852 (4,890)	-2,772 (4,158)	103 (4,110)	-613 (4,227)
Newark	2,887 (4,862)	3,123 (5,797)	2,364 (5,133)	2,930 (5,432)	1,770 (5,237)	642 (6,008)	-149 (6,512)	567 (5,769)	-1,881 (5,708)	-867 (5,888)
Perth Amboy	3,401 (3,441)	3,033 (3,887)	3,579 (3,442)	5,229 (3,642)	4,320 (3,511)	4,207 (4,028)	2,473 (4,366)	1,246 (3,858)	-578 (3,828)	738 (3,838)
Burlington	2,586 (3,491)	2,782 (4,081)	3,313 (3,586)	7,823 (3,806)	5,136 (3,869)	4,829 (4,208)	4,368 (4,562)	205 (4,041)	2,850 (3,888)	3,024 (4,111)

TABLE A.1 (continued)

	Dependent Variable									
Explanatory Variable	Quarter 1 Earnings Minus Base Period Earnings	Quarter 2 Earnings Minus Base Period Earnings	Quarter 3 Earnings Minus Base Period Earnings	Quarter 4 Earnings Minus Base Period Earnings	Quarter 5 Earnings Minus Base Period Earnings	Quarter 6 Earnings Minus Base Period Earnings	Quarter 7 Earnings Minus Base Period Earnings	Quarter 8 Earnings Minus Base Period Earnings	Quarter 9 Earnings Minus Base Period Earnings	Quarter 10 Earnings Minus Base Period Earnings
Deptford	3,767 (3,835)	3,204 (4,463)	6,885 (3,951)	8,242 (4,182)	5,767 (4,031)	7,108 (4,825)	5,838 (5,012)	2,808 (4,441)	1,240 (4,382)	2,707 (4,517)
Number of Observations	1,722	1,722	1,722	1,722	1,722	1,722	1,722	1,722	1,722	1,722
R^2	.06	.05	.07	.06	.07	.03	.04	.04	.05	.06

TABLE A.2

ESTIMATED WEEKS OF EMPLOYMENT REGRESSIONS USED TO CALCULATE TRAINING IMPACTS
(Standard Errors in Parentheses)

	Dependent Variable									
Explanatory Variable	Quarter 1 Weeks Minus Base Period Weeks	Quarter 2 Weeks Minus Base Period Weeks	Quarter 3 Weeks Minus Base Period Weeks	Quarter 4 Weeks Minus Base Period Weeks	Quarter 5 Weeks Minus Base Period Weeks	Quarter 6 Weeks Minus Base Period Weeks	Quarter 7 Weeks Minus Base Period Weeks	Quarter 8 Weeks Minus Base Period Weeks	Quarter 9 Weeks Minus Base Period Weeks	Quarter 10 Weeks Minus Base Period Weeks
Intercept	-9.15 (0.12)	-6.93 (0.15)	-5.40 (0.17)	-5.19 (0.17)	-5.12 (0.17)	-5.02 (0.17)	-4.52 (0.17)	-4.72 (0.19)	-4.88 (0.17)	-4.70 (0.17)
On-the-Job Training	-16.49 (12.28)	9.06 (16.26)	1.78 (17.59)	4.57 (17.66)	8.04 (17.70)	0.29 (18.11)	9.93 (18.17)	3.80 (17.87)	22.96 (17.56)	4.79 (17.93)
Classroom Training	15.71 (3.11)	18.50 (4.13)	13.85 (4.46)	15.25 (4.49)	16.81 (4.89)	15.16 (4.60)	12.11 (4.61)	15.84 (4.54)	17.15 (4.45)	19.36 (4.56)
Interactions with Classroom Training:										
Quarter 1 cohort	1.04 (0.76)	-0.85 (1.01)	-0.39 (1.09)	-0.36 (1.09)	-1.09 (1.09)	-4.02 (1.12)	-0.42 (1.12)	-0.97 (1.10)	-0.82 (1.06)	-0.90 (1.11)
Quarter 2 cohort	0.85 (0.74)	-0.80 (0.98)	0.05 (1.06)	-0.30 (1.07)	-1.56 (1.07)	-2.04 (1.10)	-1.63 (1.10)	-1.95 (1.06)	-1.86 (1.06)	-2.22 (1.06)
Quarter 3 cohort	1.05 (0.75)	-1.16 (1.00)	-0.01 (1.08)	0.13 (1.08)	-1.46 (1.06)	-1.48 (1.11)	-0.26 (1.11)	-0.95 (1.09)	-1.09 (1.06)	-1.49 (1.10)
Female	-0.30 (0.55)	0.15 (0.73)	0.05 (0.79)	1.86 (0.79)	1.84 (0.79)	0.79 (0.81)	0.29 (0.81)	0.0 (0.80)	1.10 (0.78)	0.51 (0.80)
Black	0.23 (0.67)	0.70 (0.89)	0.90 (0.97)	0.32 (0.97)	0.04 (0.97)	-0.71 (0.99)	-0.31 (1.00)	0.50 (0.98)	0.12 (0.96)	-0.18 (0.98)
Other non-white	0.20 (0.85)	0.14 (1.12)	2.10 (1.22)	1.81 (1.22)	0.86 (1.22)	0.06 (1.25)	0.04 (1.26)	0.31 (1.23)	0.56 (1.21)	-0.01 (1.24)
Age less than 35 years	-0.37 (0.58)	-0.03 (0.77)	-0.39 (0.83)	-0.14 (0.83)	0.19 (0.83)	-0.16 (0.85)	-0.13 (0.86)	-0.56 (0.84)	0.27 (0.83)	-0.10 (0.84)
Age 55 years or greater	-0.40 (0.72)	-0.31 (0.96)	-1.75 (1.04)	-2.40 (1.04)	-2.62 (1.05)	-2.90 (1.07)	-2.60 (1.07)	-2.71 (1.06)	-2.24 (1.04)	-2.82 (1.06)
Durable manufacturing	-0.87 (0.66)	-0.79 (0.90)	-1.31 (0.97)	-0.44 (0.97)	0.71 (0.98)	0.84 (1.00)	0.62 (1.00)	0.65 (0.98)	0.25 (0.97)	-0.82 (0.99)
Nondurable manufacturing	-0.01 (0.77)	0.09 (1.03)	-0.82 (1.11)	-0.22 (1.11)	1.28 (1.12)	0.57 (1.14)	0.23 (1.15)	0.41 (1.13)	-0.24 (1.11)	0.11 (1.13)
Union hiring	1.36 (1.37)	1.36 (1.82)	0.16 (1.97)	-0.49 (1.98)	0.98 (1.98)	0.36 (2.03)	0.89 (2.03)	-0.39 (2.00)	-0.12 (1.96)	1.85 (2.01)
Expect recall	1.07 (0.70)	1.59 (0.93)	2.26 (1.00)	0.86 (1.01)	0.76 (1.01)	1.82 (1.03)	2.00 (1.04)	2.16 (1.02)	1.91 (1.00)	1.93 (1.02)
Potential duration	-0.65 (0.11)	-0.81 (0.14)	-0.62 (0.15)	-0.55 (0.15)	-0.81 (0.16)	-0.50 (0.16)	-0.44 (0.16)	-0.49 (0.16)	-0.56 (0.15)	-0.82 (0.16)

TABLE A.2 (continued)

	Dependent Variable									
Explanatory Variable	Quarter 1 Weeks Minus Base Period Weeks	Quarter 2 Weeks Minus Base Period Weeks	Quarter 3 Weeks Minus Base Period Weeks	Quarter 4 Weeks Minus Base Period Weeks	Quarter 5 Weeks Minus Base Period Weeks	Quarter 6 Weeks Minus Base Period Weeks	Quarter 7 Weeks Minus Base Period Weeks	Quarter 8 Weeks Minus Base Period Weeks	Quarter 9 Weeks Minus Base Period Weeks	Quarter 10 Weeks Minus Base Period Weeks
Weekly benefit amount	-.0032 (.0069)	-.0030 (.0091)	.0030 (.0099)	-.0033 (.0099)	.0077 (.0100)	.0029 (.0102)	.0060 (.0102)	-.0018 (.0100)	.0010 (.0099)	.0017 (.0101)
Paterson	-0.77 (1.28)	3.48 (1.70)	3.61 (1.84)	0.52 (1.85)	0.37 (1.85)	2.16 (1.89)	-0.40 (1.90)	-0.83 (1.87)	-2.56 (1.83)	-1.76 (1.87)
Hackensack	-0.16 (1.03)	2.59 (1.38)	1.34 (1.48)	-0.21 (1.48)	-1.68 (1.49)	0.02 (1.52)	-1.00 (1.52)	-1.24 (1.50)	-2.89 (1.47)	-2.17 (1.50)
Jersey City	-1.29 (1.27)	-0.30 (1.89)	-1.63 (1.83)	-4.89 (1.84)	-4.78 (1.84)	-3.43 (1.88)	-3.58 (1.89)	-5.14 (1.88)	-5.52 (1.82)	-5.28 (1.86)
Butler	-1.41 (1.65)	-0.71 (2.18)	-0.17 (2.36)	-2.07 (2.37)	-1.16 (2.38)	0.28 (2.43)	-1.15 (2.44)	-2.25 (2.40)	-4.84 (2.36)	-3.31 (2.41)
Bloomfield	-1.48 (1.09)	0.03 (1.45)	0.66 (1.57)	-0.28 (1.57)	-1.09 (1.58)	-0.45 (1.61)	0.23 (1.62)	-0.10 (1.59)	-1.49 (1.56)	-1.42 (1.60)
Newark	-1.37 (1.16)	0.14 (1.53)	0.06 (1.66)	-0.55 (1.67)	-1.22 (1.67)	0.57 (1.71)	-0.17 (1.71)	-1.09 (1.68)	-0.64 (1.65)	-0.89 (1.69)
Perth Amboy	-0.48 (1.19)	2.12 (1.56)	3.05 (1.71)	1.77 (1.71)	0.36 (1.72)	0.50 (1.76)	0.69 (1.76)	-0.83 (1.73)	-1.54 (1.70)	-1.71 (1.74)
Burlington	-0.70 (1.05)	-0.42 (1.39)	-1.47 (1.50)	-1.81 (1.51)	-4.02 (1.51)	-3.23 (1.54)	-3.34 (1.55)	-2.96 (1.52)	-3.65 (1.50)	-3.25 (1.53)
Deptford	3.85 (1.17)	2.56 (1.55)	0.07 (1.68)	-1.68 (1.69)	-1.95 (1.69)	-0.60 (1.73)	-0.08 (1.73)	-0.27 (1.70)	-0.91 (1.67)	-0.90 (1.71)
Interactions with On-the-Job Training:										
Quarter 1 cohort	4.09 (3.65)	2.84 (4.84)	-0.86 (5.24)	-0.75 (5.27)	-1.97 (5.27)	-2.88 (5.38)	-0.94 (5.41)	-1.11 (5.32)	-8.42 (5.23)	-7.49 (5.34)
Quarter 2 cohort	4.45 (3.25)	4.38 (4.31)	3.74 (4.66)	2.90 (4.69)	1.18 (4.69)	2.42 (4.80)	3.18 (4.82)	1.95 (4.74)	2.52 (4.65)	-0.53 (4.75)
Quarter 3 cohort	5.16 (3.29)	3.76 (4.36)	1.68 (4.72)	1.08 (4.75)	-0.70 (4.75)	-1.54 (4.86)	-0.84 (4.88)	-1.86 (4.80)	-1.59 (4.71)	-2.45 (4.81)
Female	3.30 (2.04)	0.39 (2.70)	0.64 (2.93)	0.20 (2.94)	-1.01 (2.94)	2.37 (3.01)	0.99 (3.02)	2.86 (2.97)	2.85 (2.92)	5.00 (2.98)
Black	0.31 (2.00)	-1.85 (2.66)	-0.72 (2.87)	-0.80 (2.89)	0.42 (2.89)	0.18 (2.96)	0.56 (2.97)	0.13 (2.92)	-0.47 (2.87)	-0.67 (2.93)
Other non-white	-4.87 (3.42)	-4.06 (4.53)	-6.19 (4.90)	-4.26 (4.93)	-3.72 (4.93)	-2.90 (5.04)	-3.98 (5.06)	-4.13 (4.98)	-1.48 (4.89)	-2.03 (4.99)
Age less than 35 years	7.52 (1.83)	2.72 (2.56)	2.21 (2.77)	0.30 (2.78)	0.69 (2.79)	-0.25 (2.85)	-2.24 (2.88)	-0.05 (2.81)	0.03 (2.76)	-1.04 (2.82)

TABLE A.2 (continued)

	Dependent Variable									
Explanatory Variable	Quarter 1 Weeks Minus Base Period Weeks	Quarter 2 Weeks Minus Base Period Weeks	Quarter 3 Weeks Minus Base Period Weeks	Quarter 4 Weeks Minus Base Period Weeks	Quarter 5 Weeks Minus Base Period Weeks	Quarter 6 Weeks Minus Base Period Weeks	Quarter 7 Weeks Minus Base Period Weeks	Quarter 8 Weeks Minus Base Period Weeks	Quarter 9 Weeks Minus Base Period Weeks	Quarter 10 Weeks Minus Base Period Weeks
Age 55 years or greater	5.06 (2.93)	4.20 (3.89)	5.07 (4.21)	4.34 (4.23)	4.75 (4.24)	5.32 (4.33)	3.26 (4.35)	3.54 (4.28)	2.03 (4.20)	3.83 (4.29)
Durable manufacturing	2.44 (1.90)	0.59 (2.52)	-1.23 (2.72)	0.18 (2.74)	0.25 (2.74)	0.73 (2.80)	0.44 (2.81)	0.46 (2.77)	1.12 (2.72)	0.83 (2.77)
Nondurable manufacturing	-0.84 (3.83)	1.87 (5.21)	7.06 (5.63)	3.78 (5.66)	4.03 (5.67)	5.22 (5.80)	3.89 (5.82)	5.80 (5.72)	6.57 (5.62)	6.56 (5.74)
Expect recall	-2.87 (3.01)	0.48 (3.89)	-0.28 (4.32)	-1.14 (4.34)	-1.59 (4.34)	-0.34 (4.44)	0.17 (4.46)	-0.17 (4.38)	0.18 (4.30)	1.34 (4.40)
Potential duration	-0.24 (0.28)	-0.88 (0.38)	-0.53 (0.41)	-0.44 (0.41)	-0.43 (0.41)	-0.36 (0.42)	-0.48 (0.42)	-0.24 (0.42)	-0.56 (0.41)	-0.37 (0.42)
Weekly benefit amount	.0449 (.0240)	.0198 (.0319)	.0365 (.0345)	.0164 (.0347)	.0017 (.0347)	.0196 (.0356)	.0203 (.0358)	.0240 (.0350)	-.0129 (.0344)	.0362 (.0352)
Paterson	10.46 (4.33)	7.40 (5.74)	10.05 (6.21)	8.09 (6.25)	7.87 (6.25)	7.20 (6.40)	1.83 (6.42)	-0.17 (6.31)	-1.84 (6.20)	0.74 (6.33)
Hackensack	8.44 (4.51)	1.72 (5.98)	3.99 (6.47)	5.39 (6.50)	6.93 (6.51)	7.66 (6.66)	1.43 (6.68)	0.81 (6.57)	0.03 (6.45)	1.85 (6.59)
Jersey City	4.44 (6.85)	-6.58 (9.09)	-3.03 (9.83)	-0.63 (9.88)	-0.84 (9.89)	-2.19 (10.12)	-10.41 (10.15)	-12.25 (9.99)	-8.49 (9.81)	2.24 (10.02)
Butler	3.23 (5.27)	-1.06 (6.99)	4.36 (7.56)	4.82 (7.60)	5.80 (7.61)	7.15 (7.78)	0.06 (7.81)	-3.47 (7.88)	1.26 (7.54)	2.33 (7.71)
Bloomfield	10.23 (4.67)	0.82 (6.20)	4.24 (6.71)	5.40 (6.74)	5.87 (6.75)	4.54 (6.90)	-2.82 (6.93)	-3.21 (6.81)	-0.01 (6.69)	0.27 (6.84)
Newark	3.77 (6.49)	6.25 (8.61)	6.89 (9.31)	9.21 (9.36)	8.71 (9.37)	10.00 (9.59)	3.23 (9.62)	2.66 (9.46)	0.36 (9.29)	5.80 (9.49)
Perth Amboy	10.50 (4.35)	5.14 (5.77)	7.44 (6.24)	6.76 (6.27)	6.96 (6.28)	7.29 (6.43)	-0.03 (6.45)	0.86 (6.34)	-3.21 (6.23)	-0.17 (6.36)
Burlington	7.83 (4.55)	4.27 (6.03)	6.91 (6.52)	9.34 (6.56)	9.87 (6.56)	10.19 (6.72)	3.17 (6.74)	0.22 (6.63)	2.56 (6.51)	4.02 (6.65)
Deptford	10.05 (5.00)	7.58 (6.62)	10.77 (7.17)	10.81 (7.20)	4.52 (7.21)	8.23 (7.38)	3.84 (7.40)	2.51 (7.28)	-0.44 (7.15)	4.10 (7.31)

TABLE A.2 (continued)

	Dependent Variable									
Explanatory Variable	Quarter 1 Weeks Minus Base Period Weeks	Quarter 2 Weeks Minus Base Period Weeks	Quarter 3 Weeks Minus Base Period Weeks	Quarter 4 Weeks Minus Base Period Weeks	Quarter 5 Weeks Minus Base Period Weeks	Quarter 6 Weeks Minus Base Period Weeks	Quarter 7 Weeks Minus Base Period Weeks	Quarter 8 Weeks Minus Base Period Weeks	Quarter 9 Weeks Minus Base Period Weeks	Quarter 10 Weeks Minus Base Period Weeks
Observations	1,722	1,722	1,722	1,722	1,722	1,722	1,722	1,722	1,722	1,722
R^2	.09	.08	.06	.05	.08	.06	.04	.04	.06	.05

an individual-specific effect would enter directly into equation (A.3). If this unobserved effect were also correlated with whether an individual participated in training, then our estimate of the impact of training would be biased.

The potential bias in the training impact estimates could be in either direction. For example, if more motivated individuals were more likely to recover their pre-UI level of earnings and were also more likely to participate in a training program, then our estimate of the impact of training would be biased upward--the true impact would be smaller than our estimate. Alternatively, one could argue that excluding the individual-specific effects might bias our estimate of the training impact downward. For example, claimants who expect to be recalled are probably more likely to achieve their pre-UI level of earnings by t + 1 and less likely to participate in training. These correlations would generate underestimates of the impact of training on earnings.

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